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ORIGINAL ARTICLES.

I.—On Some Fossil Phyllopoda.

By Professor T. Rupert Jones, F.R.S., and Dr. Henry Woodward, F.R.S.

(PLATE IX.)

THESE notes on fossil Phyllopods refer to some interesting specimens, mostly of Palæozoic age, which have come to hand since the publication of Part II. of our "Monograph of the British Palæozoic Phyllopoda" (Palæontographical Society), 1892.

Figs. 1 and 2 represent *Estheriæ* from the upper part of the great Karoo Formation of South Africa. Figs. 3 and 4 are exposed on a piece of Moffat shale from Dumfries-shire; and, Fig. 3 being a *Discinocaris*, it is probable that the two series of abdominal segments, Figs. 4*a*, 4*b*, may have belonged to that genus. Fig. 5 is a *Discinocaris* of good size, and Fig. 6 an *Aptychopsis*, both from Moffat. Fig. 7 is a rare Phyllocarid from the Devonian beds (Hamilton series) of Canada. There is also a note on a Nova-Scotian *Estheria*, without figure.

1. Estheria Draperi, sp. nov. (Pl. IX. Figs. 1*a*, 1*b*, 1*c*.)

Size: Length of valve, 16 mm. Length of hinge-line, 11 mm. Height, 10½ mm.

Valves suboblong, straight above, slightly curved below, rounded at the ends; anterior margin higher and less convex than the posterior. The interspaces on the surface are ornamented with coarse shallow pits, making an obscure reticulation (see Fig. 1*c*, which was taken from near the edge of the antero-ventral portion of the left valve, lying uppermost). The umbo is just in front of the middle of the hinge-line.

The left valve, partly preserved and much corrugated, lies on top of a slightly convex mass of dark shale, which represents the major part of the body of the animal.

In shape this specimen approaches the Permian *Estheria exigua* of Russia (Monog. Foss. Estheriæ, Pal. Soc. 1862, p. 37, pl. i. figs. 22–24) and the recent *E. Rubidgei*,[1] *E. Macgillivrayi*,[2] and *E. Dahalacensis*[3]; but has its umbo less forward than either of these. Its ornament approaches that of the last-mentioned species, and also that of *E. exigua*.

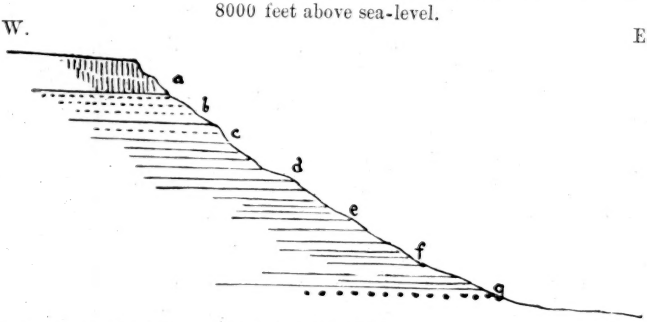
[1] Proc. Zool. Soc. 1862, p. 148, pl. xv. fig. 3. [2] *Ibid.* fig. 5.
[3] Proc. Zool. Soc. 1849, Annulosa, p. 87, pl. xvii. figs. 2–4.

From a small bed of grey shale interstratified in the Cave-Sandstone in the uppermost part of the Karoo Formation, in Natal, above the homestead of Mr. Theodore Malder, on the eastern side of the

Fig. 1.

Section of the Eastern side of the Platberg in the Drakensberg.

8000 feet above sea-level.



a. Columnar Dolerite. About 400 feet thick.
b. Cave-Sandstone (with the Estherian shale). About 400 feet thick.
c. The place of the Red Beds, which are fossiliferous at Harrismith, on the western side of the Platberg, in the Orange-Free-State.
d. Molteno Beds, bearing coal in the Stormberg, High-veldt, and Natal.
e. Beaufort Beds.
f. Ecca Beds. (*c*–*f*. about 2000 feet.)
g. Ecca (Dwyka) Conglomerate, lying nearly horizontal in the adjacent valley of Pietermaritzburg, Natal.

Platberg (see the Section, Fig. 1). This is a part of the Drakensberg range, and on its western side is Harrismith in the Orange-Free-State. This *Estheria* was found by Mr. David Draper, F.G.S., of Lennoxville, Natal; and is here named after him. It is deposited in the British Museum (Natural History).

2. Estheria Stowiana, sp. nov. (Pl. IX. Figs. 2*a*, 2*b*.)

Size: Length of valve, 2½ mm. Length of hinge-line, 2 mm. Height, 1½ mm.

On the piece of shale bearing *Estheria Draperi* (Fig. 1) are two very small flattened Estherian valves, one of which is shown, much enlarged, by Fig. 2. Their shape is different from that of the larger form, being sub-elliptical, much less oblong, relatively higher and more boldly curved at one end, and elliptically curved on the ventral border; with the umbo nearer to the narrow than to the broader (higher) end of the valve.

It is unusual for the umbones, and consequently the anterior part of the body, to be nearer to the smaller than to the larger end of the valves. This condition is seen in some varieties of *E. striata* (Monog. Foss. Estheriæ, 1862, pl. i. figs. 11, 13, and 15), in *E. Dawsoni* (Geol. Mag. 1870, Pl. IX. Fig. 15), and in *E. tenella*, var. (*ibid.* Fig. 16); but their umbones are much further forward than in our Fig. 2, where it is not far in front of the middle of the hinge-line. The form nearest to our Fig. 2, in this submedian

position of the umbo, is the Carboniferous *E. striata*, var. *tenuipectoralis*, from the Ural (Ann. Mag. Nat. Hist. ser. 5, vol. xii. 1883, p. 246, pl. vi. fig 2). In this form, however, the antero-ventral margin has less convexity and the postero-ventral curvature is more oblique.

In *E. membrancea* (Monog. Foss. Esth. pl. i. figs. 3 and 6), although the front and ventral margins do not make so elliptical a curve, the umbo is in relatively the same position as in our Fig. 2. *E. Greyii*, from the Karoo Formation near Cradock, South Africa (Geol. Mag. 1878, p. 100, Pl. III. Fig. 1) has a somewhat similar position for its umbo, but the dorsal margin is not straight.

Although it is just possible that these small specimens may be *young* forms, differing according to their stage of growth from such an adult as Fig. 1 (with which they were associated), yet, not having evidence of such a relationship, we propose to distinguish them by a separate name, and to call them *E. Stowiana*, in memory of the late G. W. Stow, who did much to elucidate the structure and history of the Karoo Beds, including the Cave-Sandstone, in a shale-bed of which these *Estheriæ* occur.

3. Discinocaris Browniana, H. Woodward, 1866.
(Pl. IX. Figs. 3–5.)

Discinocaris Browniana, H. W. and T. R. J., Monog. Pal. Phyll. Part II. 1892, p. 119, pl. xvi. figs. 12–19, 21–23.

Fig. 3. Height (length), 8 mm. Breadth, 8 mm. This is a carapace modified by pressure, slightly concave, and apparently overlying a fragment of another, which shows through the nuchal notch, or the nuchal portion has been displaced and turned partly round in the notch.

From Moffat; with the Graptolites—*Rastrites peregrinus* and *Climactograptus* (?).

Figs. 4*a*, 4*b*. Portions of two abdomens; 4*a*, 13 mm. long, and showing 12 segments, figured with the proximal end downwards to the right hand; and Fig. 4*b*, less distinct, showing 7 segments.

These are not unlike several examples of larger body-segments of *Hymenocaris*, figured in pl. xiii. of Monog. Foss. Phyll. Part II. 1892, and described at p. 79; but, as *Hymenocaris* does not occur in the Moffat shales, nor *Discinocaris* at Tremadoc, we must take for granted that these abdominal relics belong to different genera. So also there are in the British Museum other similar sets of body-rings, not so large as those of *Ceratiocaris stygia* and *C. papilio*, but belonging to smaller forms of that genus. Such examples were described by us in Monog. Foss. Phyll. Part I. 1888, p. 56, and one (*C. laxa*) was figured on pl. 8. fig. 12.

This genus also is foreign to the Moffat shales, and therefore cannot claim Figs. 4*a*, 4*b*, for itself. Indeed such segmented abdomens may and must have belonged to all the above-mentioned and to other Phyllocarids.

Fig. 5. Height (length), 10 mm. Breadth, 12½ mm. This is a rather large and characteristic carapace of *D. Browniana*, well

preserved in some respects, but slightly concave and cracked by pressure, and somewhat damaged along the upper edge.

From Garpel Linn, Moffat.

4. Aptychopsis Wilsoni, H. Woodward, 1872. (Pl. IX. Fig. 6.)

Aptychopsis Wilsoni, T. R. J. and H. W., Monog. Foss. Phyll. Part II. 1892, p. 105, pl. xv. figs. 12 (?), 15, and 16.

Half of a carapace: Height (length), 14 mm. Breadth, 8 mm.

This left-hand moiety of a carapace of good size lies apparently over a displaced fragment of another part of the carapace, which interferes with the angular shape of the nuchal notch.

From Duff-Kinnel Burn, Moffat.

Figs. 3, 4, 5, and 6 were lent by the authorities of the Carlisle Museum, for examination and description, through the courtesy of R. S. Ferguson, Esq., Lowther Street, Carlisle.

5. Elymocaris Hindei, sp. nov. (Pl. IX. Fig. 7.)

Length of valve, 24 mm. Height, 9 mm.

We have here two valves of a carapace. The right valve is nearly perfect, but has lost a piece off the hinder end, and is somewhat cracked by pressure. The other valve lies obliquely and partly embedded.

The best-preserved valve is elongate, pod-shaped, straight above, with a nearly semicircular outline below; antero-dorsal angle sharp; posterior end of the valve tapering, but fractured,—certainly not

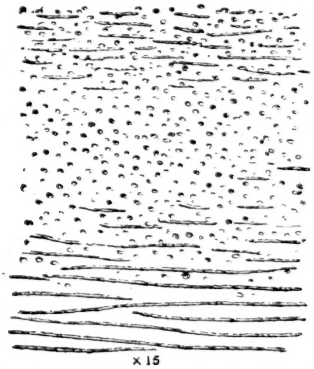


Fig. 2.—Ornament on the valve of *Elymocaris Hindei*: magnified 15 diameters.

broadly truncate originally. A distinctly elevated ocular spot is visible at about one-seventh of the length of the valve, a little below the margin, in the antero-dorsal region. Delicate longitudinal striæ, parallel with the margin, interrupted, and slightly anastomosing, ornament the ventral part of the surface. This pattern passes upwards into scattered minute shallow pits; and above the middle of the valve there is a linear arrangement of fine short striæ, with shallow pits interspersed (see Fig. 2).

In some features this carapace resembles *Emmelezoe elliptica* (M'Coy), Monog. Foss. Phyll. Part I. 1888, p. 68, pl. 8, fig. 1; but it is still more closely allied to *Elymocaris siliqua*, C. E. Beecher, Second Geol. Surv. Pennsylvania, P.P.P. 1884, p. 13, pl. 2, figs. 1 and 2. In size it is nearer the latter, which measures 26 by 10 mm., whilst the former measures 32 by 13 mm. It differs from both in being evidently more attenuate posteriorly (though not quite perfect); and more symmetrical in the elliptical curve of the ventral margin; and from *Emmelezoe* particularly in its frontal angle. As in *Emmelezoe*, there are no nodular eminences near the eye-spot.

Its nearest ally, however, is evidently *Elymocaris capsella*, Hall and Clarke (Palæont. New York, vol. vii. 1888, p. 181, pl. 31, fig. 4), as pointed out to us by Dr. Hinde when communicating the specimen; and, although this is smaller (15·5 by 5 mm.), its proportions are not far from being the same; and its tapering posterior extremity, its striate ornament, and general shape give a close resemblance to our Fig. 7. The greater fulness of the antero-ventral, and less convexity of the postero-ventral, margin in *E. capsella*, and the steeper front edge, with its antero-dorsal spike, distinguish it from the species before us; besides which the latter has a more prominent ocular spot.

Regarding it, then, as belonging to *Elymocaris*, and distinct from the published species of the genus, we name it *E. Hindei* after Dr. G. J. Hinde, V.P.G.S., who collected it many years since at Arkona, Ontario, Canada, from the Hamilton Group of the Middle Devonian Series.

6. Estheria Dawsoni, Jones, 1870.

Acadian Geology, 1868, p. 256, fig. 78*d*; Geol. Mag. 1870, p. 220, Pl. IX. Fig. 15; *ibid.* 1876, p. 576; *ibid.* 1878, p. 101, Pl. III. Fig. 2.

We have just now received from Sir William Dawson, F.R.S., etc., of Montreal, a fossil *Estheria*, which, though imperfect, corresponds in general characters with *E. Dawsoni*, Jones, and moreover exhibits an interstitial pitting or coarse reticulation, such as is present in several known *Estheria*, but rendered obscure by fossilization. "The shell enclosed" (writes our friend, in a letter dated April 28, 1894) "came to me, with some fossil plants, from beds in Nova Scotia, which I suppose to be Lower Carboniferous, but which some hold to be Devonian." He suggests that it looks like the species here referred to.

E. Dawsoni has been found in the Lower Carboniferous series at Horton in Nova Scotia, and in the Lower Carboniferous of Scotland.

EXPLANATION OF PLATE IX.

Fig. 1. *Estheria Draperi*, sp. nov. 1*a*, carapace with the left valve, partly preserved, lying upwards; enlarged 2 diameters. 1*b*, outline, natural size. 1*c*, portion of the ornament, magnified 25 diameters. Eastern side of the Platberg, Natal.

Fig. 2. *Estheria Stowiana*, sp. nov. 2*a*, right valve, magnified 15 diameters. 2*b*, outline, nat. size. Eastern side of the Platberg, Natal.

Fig. 3. *Discinocaris Browniana*, H. W. Modified by pressure. Moffat. Magnified 2 diameters.

Fig. 4*a*, 4*b*. Portions of two bodies or abdomens of, probably, *D. Browniana*. On the same shale with Fig. 3. Magnified 3 diameters.

Fig. 5. *Discinocaris Browniana*, H. W. Modified by pressure. Garpel Linn, Moffat. Magnified 2 diameters.

Fig. 6. *Aptychopsis Wilsoni*, H. W. Left-hand moiety of a carapace. Duff-Kinnel Burn, Moffat. Magnified 2 diameters.

Fig. 7. *Elymocaris Hindei*, sp. nov. Magnified 1½ diameters. Devonian; Arkona, Ontario, Canada.

II.—Restoration of *Elotherium*.

By Prof. O. C. Marsh, M.A., Ph.D., LL.D., F.G.S.;
of Yale College, New Haven, U.S.

(PLATE X.)

THE genus *Elotherium*, established by Pomel in 1847, represents a family of extinct mammals, all of much interest. They were first found in Europe, but now are known in the Miocene of North America, not only on the Atlantic coast, but especially in the Rocky Mountain region, and still further west. This family includes several genera, or subgenera, and quite a number of species, some of which contain individuals of large size, only surpassed in bulk among their contemporaries by members of the Rhinoceros family, and of the huge *Brontotheridæ*.

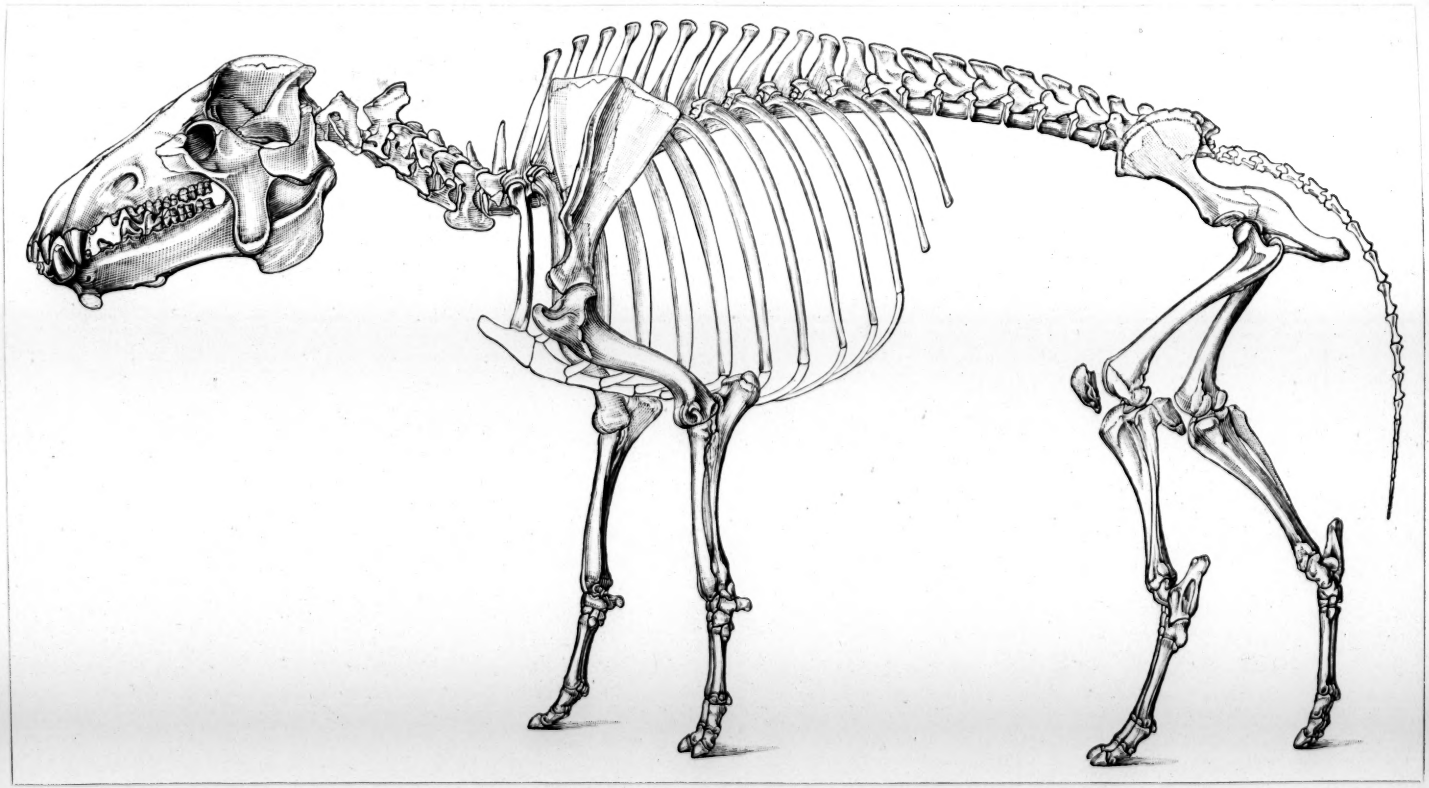
Remains of this group have thus been known for nearly half a century, yet, until recently, comparatively little had been determined with certainty regarding the skeleton, or of the skull except the dentition, although Aymard, Leidy, Kowalevsky, and others have made interesting contributions to the subject. In a late paper[1] the writer gave figures of a finely preserved skull, and also of a fore and hind foot, of one of the largest species, *Elotherium crassum*, Marsh; and in the present article an attempt is made to restore the entire skeleton of this animal, to serve as a typical example of the group.

The restoration, one-twelfth natural size, given on Plate X., represents a fully adult individual, which, when alive, was more than seven feet in length and about four feet in height. The basis of this restoration is the type specimen of *Elotherium crassum*, which was found by the writer in 1870, in the Miocene beds of North-Eastern Colorado, and described in 1873.[2] A number of other specimens since obtained in the same region, and still others from essentially the same horizon in South Dakota, all evidently pertaining to this species, were likewise used in the restoration.

The type specimen, although incomplete, includes portions of the skull, with various vertebræ and bones of the limbs and feet, and these were sufficient to determine the general form and proportions of the animal here restored. The additional specimens used are mostly in good preservation, and some of them are almost as perfect as in life. Hence, the skeleton, as represented in Plate X., is believed to be correct in all its essential features.

[1] American Journal of Science, vol. xlvi. p. 408, pl. viii. November, 1893.

[2] *Ibid.* vol. v. p. 487, June, 1873.



Restoration of Elotherium crassum, Marsh. One-twelfth natural size.
From the Miocene Beds of North-Eastern Colorado, U.S.A.

Looking at the skeleton, as here shown, it is evident that the most striking features are the large and peculiar skull, and the elongate and slender limbs and feet—characters that do not in themselves suggest the suilline affinities of the animal, which a closer study brings to light. The most notable points in the skull, as here indicated, are the long, pendent process of the malar bone, characteristic of some of the sloths, and the strong projections on the lower jaw. The latter supplement the malar process, but are developed to a greater degree than in any other mammals. Another feature of the skull to which the writer has already called attention is the very small brain-case, which proves that the brain itself was very diminutive. This was also true of the other known species, and was probably the main reason which led to the early extinction of the whole group.

The slender, highly specialized limbs and feet are likewise particularly noticeable in the restoration. They indicate clearly that the animal was capable of considerable speed, and this must have been of great service as a protection from its enemies. It will be seen that in each foot there are only two functional digits, corresponding to the third and fourth in man. The first digit is entirely wanting, and only remnants remain of the second and fifth.

Such reduction was, of course, a gradual process, extending over long geological periods. It indicates clearly a change of environment from the swampy home of the primitive five-toed suilline to the elevated, firm upland of later times, over which the present species and its near allies doubtless roamed. A parallel instance, still more striking, is seen in the gradual change which took place in the equine mammals, as first shown by the writer more than twenty years ago.[1]

The *Elotheridæ* were evidently true suillines, but formed a collateral branch that became extinct in the Miocene. They doubtless branched off in early Eocene time from the main line, which still survives in the existing swine of the old and new worlds.

III.—On *Temnocheilus coronatus*, M'Coy, from the Carboniferous Limestone of Stebden Hill, near Cracoe, Yorkshire.

By Arthur H. Foord, F.G.S., of the Royal Dublin Society, Dublin; and G. C. Crick, Assoc. R.S.M., F.G.S., of the British Museum (Natural History).

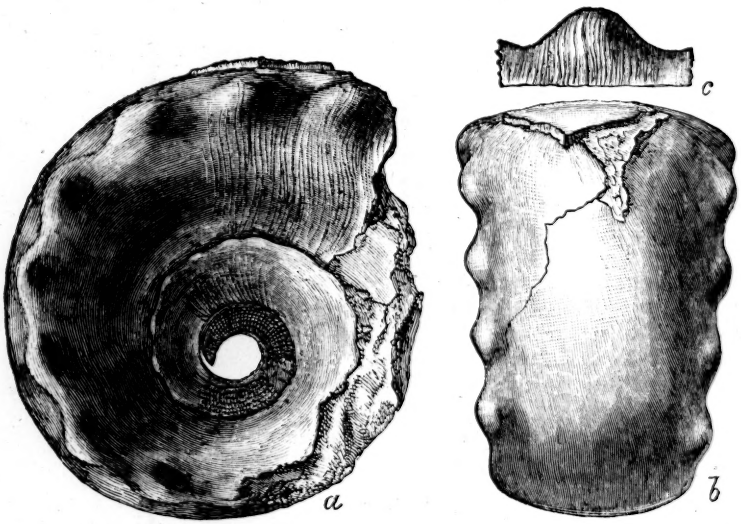
THE specimens which form the subject of this paper were obtained by Mr. E. J. Garwood from the Carboniferous Limestone of Stebden Hill, near Cracoe, Yorkshire; and the writers are indebted to the kindness of Mr. Garwood for permission to describe them. Respecting the horizon and locality from which the species came, Mr. Garwood says, in a letter to one of the writers: "Stebden is a hill which Tiddeman [of the Geological Survey] describes as one of his knoll reefs, and is, in my opinion, the equivalent of the Upper Scar limestone on the south side of the Cracoe fault, where it is much more fossiliferous than on the north

[1] American Journal of Science, vol. vii. p. 257, March, 1874.

(*i.e.* Pen-y-gent, Ingleboro, etc.)." Mr. Garwood adds: "Both of the specimens were much larger, when first exposed in the matrix; but the rock was so jointed, and the outer whorl [body-chamber?] so hollow, and replaced by calcite crystals, that only fragments of this could be obtained, and a good deal was broken before I could discover the extent of the fossil *in situ*."

The close resemblance of the present form to the *Temnocheilus coronatus* of M'Coy will be apparent to any one acquainted with that species, either through the figure given of it in the "Synopsis of the Carboniferous Fossils of Ireland" (pl. iv. fig. 15), or by direct knowledge of the specimen itself in the Museum of Science and Art, Dublin. M'Coy's specimen, which came from the Carboniferous Limestone of Little Island, near Cork, is, however, greatly distorted by cleavage, and is apparently immature.

In his "British Palæozoic Fossils" (p. 557), M'Coy referred to this species some examples from the Carboniferous Limestone of Lowick, Northumberland, and these enabled him to amend his original description of the species. These specimens are now preserved in the Woodwardian Museum, Cambridge; one is fairly complete and four are fragmentary. They are all internal casts and



Temnocheilus coronatus, M'Coy.—*a*, lateral view of inner whorls of an example about 130 mm. in diameter; *b*, peripheral view of the same; *c*, tubercle of another specimen at a diameter of about 120 mm., showing the coarse lines of growth. *a* and *b* are slightly reduced, *c* is of the natural size.

bear only fragments of the test; but upon the inner whorl of the tolerably complete example there is an indication of the longitudinal ornamentation of the test, and this is just as in M'Coy's type-specimen; moreover, the dimensions and other characters of these examples agree so closely with M'Coy's type, that we think there

cannot be any doubt as to their identity. But Mr. Garwood's specimens (there are two) are in such a fine state of preservation, and probably nearly, if not quite, mature, that they enable us to add somewhat even to M'Coy's amended description of the species. One specimen has a diameter of 125 mm.; the other, the inner whorls of which are represented in the accompanying figure (the outer being omitted to save space), was somewhat larger.

The shell consists of rather more than three whorls, and there is a central elliptical vacuity whose diameters are 10 and 7 mm. respectively. The septa are about 10 mm. apart on the periphery, where the shell has a diameter of 90 mm. The ornaments consist of a row of tubercles situated at the junction of the umbilical and peripheral borders. Beginning at about the mid-length of the first whorl as obscure undulations, the tubercles gradually increase with the growth of the shell, until at about the second whorl they have become large and prominent. They are of a flattened conoidal form and slightly elongated longitudinally. There are fourteen tubercles per whorl in an adult shell. The test is thin, its surface marked with fine striæ of growth, forming an obscurely sigmoid curve upon the sides, and bent backwards in a deep sinus on the periphery, where they become much coarser in the adult shell, especially in crossing the tubercles (see Figure). The ornamentation of the young shell is that which is characteristic of most, if not of all, of the coiled Nautiloids, including the recent *Nautilus*, viz. fine longitudinal raised lines, crossed by still finer ones, and thus forming a beautiful cancellated structure. The transverse lines become obsolete before the end of the first whorl, but the longitudinal lines die out at about the end of the first fourth of the second whorl, among the last to disappear being two lines just below the tubercles.

Some differences are observable between M'Coy's type and the Yorkshire form, the peripheral area in the former being a little wider than that of the latter, and the sides of the umbilicus rather steeper; the tubercles also are more flattened than they are in the English fossil. These differences, however, may be rightly attributed, we think, to the distortion undergone by M'Coy's specimen, the lateral pressure having widened the periphery, while at the same time altering the true form of the umbilical region. M'Coy's figure (pl. iv. fig. 15) represents a more perfect specimen than the one from which it was drawn, unless the latter has since been damaged. The only species with which M'Coy, when describing his species, compared it is the *Nautilus* [=*Cœlonautilus*] *cariniferus*[1] of J. de C. Sowerby, tuberculated species of the group now called *Temnocheilus* being probably rare at the time M'Coy wrote. But in his "British Palæozoic Fossils" (p. 558), M'Coy in referring to this species says: "This exceedingly rare species is distinguished completely from the *N. tuberculatus*[2] by the great thickness, or width, of the mouth as compared with the diameter, the much more rapidly

[1] J. de C. Sowerby, Min. Con. vol. v. p. 130, pl. cccclxxxii. fig. 3 (excl. fig. 4).
[2] J. Sowerby, Min. Con. vol. iii. p. 90, pl. ccxlix. fig. 4.

enlarging whorls, much deeper and narrower umbilicus, direction of the flattening of the tubercles, acutely elliptical form of the transverse section of the whorls, and forward instead of backward wave of the edge of the septa at the middle of the periphery." We have examined J. Sowerby's type of *Nautilus tuberculatus*, which is in the British Museum (Nat. Hist.), and have no doubt that it is specifically distinct from M'Coy's species.

De Koninck,[1] in describing the present species from Visé, Belgium, compares it with *N. biangulatus*, J. de C. Sowerby [?= *Cœlonautilus cariniferus*, J. de C. Sowerby], and with *Nautilus tuberculatus*, J. Sowerby, and *Nautilus latus*, Meek and Worthen. He distinguishes M'Coy's species from Sowerby's and Meek and Worthen's by its smaller size, the greater relative height of its aperture, the depth of its umbilicus, and the form of its septa. As far as can be judged by the figures there seems much to justify de Koninck's identification of the Visé fossil with *Temnocheilus coronatus*; and Mr. Garwood in a letter to one of the writers expresses the same view, he having seen the specimens in the Natural History Museum at Brussels last autumn.

There is a small but well-preserved specimen in the Woodwardian Museum, Cambridge, from the Carboniferous Limestone, Settle, Yorkshire, which we think belongs also to this species.

Finally, our best thanks are due to Mr. W. W. Watts, of the Geological Survey, through whom we obtained access to Mr. Garwood's specimens, and to Prof. Hughes and his assistants, Messrs. Woods and Reed, for their kindness in allowing one of us to examine M'Coy's types in the Woodwardian Museum, Cambridge.

IV.—Jurassic Ammonites: Notes on a Pamphlet by Professor Guido Bonarelli.[2]

By S. S. Buckman, F.G.S.

ITALIAN geologists have made several valuable contributions to our knowledge of the Jurassic strata of their country. Unfortunately some of their works are not known here as they deserve to be, and seem difficult to obtain. At the same time I have to acknowledge with thanks the receipt of some valuable memoirs which have been of particular service; and one of the latest arrivals is the present pamphlet.

This memoir deals with what has usually been called "Upper Lias" and "Lower Inferior Oolite"; but these terms the author proposes to replace by "Toarcian" and "Aalenian." He deals, first, with the palæontology, and mentions various well-known species, some of which we are familiar with in this country, but others are peculiar to the districts adjacent to the Mediterranean. In this connection it may be noted that *Hammatoceras* in a great measure, and

[1] Faune du Calcaire Carbonifère de la Belgique (Annales du Mus. Roy. d'Hist. nat. de Belgique, vol. ii.), part i. 1878, p. 115, pl. xxiv. ff. 2*a*, *b*.

[2] Osservazioni sul Toarciano e l'Aleniano dell' Apennino centrale. Boll. d. Soc. geol. italiana, vol. xii. fasc. 2, pp. 195–254. Rome, 1893.

Erycites especially, belong to the Mediterranean regions; while the Lytoceratites[1] were particularly abundant—in fact, individually and specifically they would seem to have flourished, in the Mediterranean districts as compared with this country, as ten to one.

Two new genera are introduced in the present pamphlet, namely, *Paroniceras* (Prof. Parona) and *Collina* (derivation not given, but perhaps from *Collinus*, "of a hill"). The type of *Paroniceras* is *Am. sternalis* (von Buch); and concerning its value in our opinion see the appended paper on "The genus *Cymbites*." The type of *Collina* is a new species, "*gemma*," which may be described as a carinate *Dactylioceras* allied to *mucronatum* (d'Orbigny). The carination appears to be the feature which separates *Collina* from *Dactylioceras*; and if it can be shown that *Dactylioceras* split up into two branches, one of which developed a carina, and the other remained uncarinate, the genus *Collina* (? *Collinus*) will have good foundation.

Among specific names to be noted are, first, the application of Oppel's "*iserensis*" to *Am. erbaensis* (Dumortier, *non* Hauer), a very necessary correction; and, second, the new name "*Cœloceras? Mariotti*" applied to *Am. Humphriesianus nodosus* (Quenstedt, Jura, pl. 54, fig. 4). This name, however, is superfluous, and must rank as a synonym; because Hyatt has already applied the name "*nodosum*" to this very figure: he called it "*Stephanoceras nodosum*."[2] It may be remarked that, in translating Quenstedt's trinomial or quadrinomial system into modern currency, the first two names, say "*Ammonites Humphriesianus*," are really generic, and are concisely written "*Stephanoceras*" or "*Cœloceras*," while the third name thus becomes specific; and, unless the restricted genus already contain the same name, Quenstedt's third name should always be used. Further, both *Cœloceras* and *Dactylioceras* have priority of *Stephanoceras*, while a name *Stephanoceros* was already in use for a genus of Rotatoria.[3]

Prof. Bonarelli places as Toarcian the *Falciferum-* to *Jurense*-zones, and as Aalenian the *Opalinum-* to *Concavum*-zones. He discusses the division between "Lias" and "Jura," and his conclusion is thus expressed (p. 254): "In the Central Apennines the most natural division between Lias and Jura which it is possible to adopt is precisely that proposed by Münster and upheld by Vacek." That

[1] I propose this name as a sort of colloquial word, congruous with Ammonites, Ceratites, Goniatites, etc., to distinguish those genera which are certainly not true Ammonites, either in characters or by descent, namely, *Lytoceras*, *Pleuracanthites*, *Phylloceras*, *Monophyllites*, etc. They are characterized by the phylliform cells of the septa, and probably by the absence of any calcareous Aptychus or Anaptychus. Lytoceratites are biologically lower than Ammonites, though they are collateral and coeval. They were separated by Hyatt (Genesis Arietidæ, p. 4) as *Lytoceratinæ*; but this places *Lytoceratinæ* superior to *Lytoceratidæ*, whereas it should be of inferior rank. The terms *Lytoceratacea* and *Ammonacea* (the latter used many years ago) might indicate Lytoceratites and Ammonites respectively, and be subdivisions of *Ammonoidea*.

[2] "Genetic Relations of *Stephanoceras*," Proc. Boston Nat. Hist. Soc. vol. 18, p. 386, 1876.

[3] Hyatt, *op. cit.* p. 368 (E. B. Tawney's information).

is to say, up to the *Concavum*-zone, inclusive, is "Lias," above it "Jura." It may be remarked that no local lithological details, whether in the Central Apennines or in this country, can have the least consideration in regard to the division ultimately adopted. It must be settled solely on palæontological data; and the following facts may be noted, that, below the *Concavum*-zone, the *Arietidæ* and *Hildoceratidæ* (practically the carinate Ammonites) are dominant; after that date the *Stephanoceratidæ* hold the field. This distinction is of European, and possibly of worldwide, application; while a lithological test is of merely parochial usefulness—sometimes not even that.

V.—Note on Sediments Dredged from the English Lakes.

By W. Maynard Hutchings, F.G.S.

DR. H. C. MILL has recently made elaborate bathometric surveys of the principal English lakes, and whilst taking the soundings he has collected a series of samples of the bottom-deposits, for the purpose of microscopic study.

At the request of Mr. Marr I undertook a careful mineralogical examination of these samples, and, though nothing very striking or specially interesting has resulted from the work, it may be worth while to record in the Geological Magazine the fact of such an examination having been made, and to state the mineralogical and other observations noted.

After some preliminary experiments, the method of dealing with the sediments finally adopted was as follows. Each sample was passed through a sieve of 130 holes per linear inch, and only the material fine enough to pass this mesh was submitted to microscopic examination at all. From this first portion a second was separated by a sieve of 250 holes per inch, and from this again a fine slime was washed out by levigation in beakers; also a separation of the heavier minerals of the deposits was made by means of a solution of Sp. G.=3. The four grades of material so obtained were examined mounted in balsam and in water.

I have been much struck by the fact that most of the samples show great tenacity when dried, and great plasticity when made into a paste with water. This high degree of tenacity and plasticity was unexpected, in view of the large amount of quite angular and gritty material contained, and the relatively small proportion of mineral matter in a sufficiently fine state of division to be compared to the ordinary clays. Partly, however, these qualities are due to a considerable amount of organic matter of vegetable origin, and to abundant diatoms and other mineral organisms and their remains.

The mineralogical composition of the deposits is, in the main, exactly what would be expected from a knowledge of the rocks in the respective areas drained, though there are one or two examples of minerals which occur in the sediments in greater abundance than would have been looked for, as *e.g.* hornblende, both green and brown, occurring in lakes receiving the drainage of areas of

"volcanics," though that mineral is rarely recorded as occurring in the rocks in question.

Also there is no practical difference to be observed in the nature of the deposits in different lakes which receive the waters from areas of similar general nature. Bassenthwaite, however, which receives the waste of an area of sedimentary rocks, with an almost total exclusion of "volcanics," has a deposit very different in nature from those of the other lakes.

Taking the lakes which receive the drainage of volcanic areas pure and simple, the minerals identified are:—

Quartz.
Felspars.
Garnet.
Rhombic Pyroxene (very scarce and only in Wastwater).
Hornblendes.
Muscovite.
Biotite.
Epidote.
Chlorite.
Calcite.
Zircon.
Tourmaline. } in clastic fragments only.
Rutile. }
Hematite.
Magnetite.

Amongst the felspars the varieties identified are orthoclase, albite, labradorite, anorthite, and a few bits of microcline.[1]

[1] The felspar-fragments in these deposits are not well suited for identification of varieties, as they are to a great extent quite irregular, ragged bits, few of them showing any definite boundaries or cleavages with which to correlate extinctions and optic figures. The larger bits ($\frac{1}{150}$ inch) are mainly more or less turbid, like most of the felspars in the rocks from which they are derived; but among the smaller grains of the more finely-sifted materials there are plenty of clear fresh bits to be found.

It may be remarked, in this connection, that even with very imperfect material, both in a fragmental condition and in rock-sections, a good deal more can be done in the way of identification than is often supposed, by making use of Schuster's method of extinctions, in conjunction with a careful application of the figures in convergent polarized light. It is often not necessary to have cleavage-flakes sufficiently good to enable the *sign* as well as the *angle* of extinction to be determined. Thus, suppose we have a very imperfect flake, or an irregular bit in a slide, showing, however, one good cleavage-crack or boundary. If we find the extinction is 18°—20°, measured against this line, and if we have at the same time an undoubted emergence of a *positive bisectrix* not quite central in the field, then we know that we have albite. A similar fragment or bit in a section, giving an extinction of about 36°, with a sharp and distinct axial bar well within the field, but near its edge, will be anorthite. Other determinations may be made in this way on very unpromising material, provided the optic figures are understood and are carefully used as a guide, and no attempt is made to draw inferences except when these figures can be determined beyond question.

This is not an appropriate place in which to go more fully into this subject, which can be easily followed out by studying the details given in Rosenbusch, or in Teall's "British Petrography" or other works, as to Schuster's method, and the optic figures on the M flakes of the different felspars.

To obtain good results it is necessary, as I pointed out in previous remarks on the determination of felspar (Geol. Mag. January, 1894, p. 41), to have efficient optical apparatus. In very thin flakes, or in bits in thin sections, owing to the very low polarization-tints, it is often difficult to determine the extinctions with sufficient

The hornblende is partly green and fibrous, such as results from the alteration of augite; but a good many bits of the deep-brown compact variety occur, more probably of original volcanic origin.

The muscovite is of two kinds. In the finer portion of the materials the mica is abundant as small, usually greenish flakes of sericitic nature, such as one sees plentifully in the altered felspars, and in the mass, of the lavas and ashes of the district. In the larger-grained portions of the deposits examined there are larger flakes of muscovite than are seen in these rocks.

The biotite is all of this larger kind. It is to be recollected that the deposits contain the waste not only from the rocks of the district *in situ*, but also from the materials of the surface-drift, and it is to this latter that we must probably refer some of the minerals found, as notably the microcline and biotite and some of the larger flakes of muscovite, though in the case of Wastwater these latter minerals may have come from granitic rocks *in situ*.

Taking Bassenthwaite as representing the deposits from an area of slate-rocks, we still find closely the same *qualitative* list of minerals, but a totally different *quantitative* arrangement of them. Thus, felspar is scarce, and among it plagioclase is rare. Chlorite is also scarce, in contrast to its great abundance in the deposits from the "volcanics." The great bulk of the material consists of flakelets of slate, showing quartz and mica, and crowded with minute rutile-needles. The finest portions consist mainly of the minute scales of mica from these slates, with the rutile-needles and with a good many of the small anatase crystals characteristic of clays and shales.

Tourmaline is present, not only as clastic grains as in the other lakes, but also as the small green hemihedral crystals characteristic of the slates, etc. In the deposits from the "volcanics" the quartz, like the other minerals, is all present as angular fragments or flake-like chips; the distance it has traversed was too short to allow of any notable amount of rounding of the grains. The quartz in Bassenthwaite is largely in a more smooth and rounded form; it represents the grains which were already well worn when they were deposited as part of the materials now forming the Skiddaw slates, etc.

In Derwentwater we have a lake giving a mixed deposit, in which both "volcanics" and "sedimentaries" take part. The former, however, much predominate in an average sample for the entire lake, and the latter are only in evidence by a very few minute slate-fragments, by rutile-needles and by small crystals of tourmaline.

accuracy. For this purpose a Bertrand quartz-eyepiece (with four quartz segments) will be found of the greatest service.

The discrimination of the felspars is, of course, of the highest importance in all petrological work, and especially so in the study of metamorphic rocks, so that too much stress cannot be laid upon the desirability of carrying it out in all cases as fully and as definitely as the nature of the material will allow. It appears to be sometimes left undone, or very vague, where there is no necessity to so leave it; or it is even left undecided whether a mineral is quartz or felspar,—an uncertainty which is seldom necessary where the mineral or minerals occur in any reasonable quantity in a slide.

VI.—Notes on Russian Geology.[1]

By W. F. Hume, D.Sc., A.R.S.M., F.G.S.;
Demonstrator in Geology, Royal College of Science.

III. The Black Earth.

In a previous paper on the Loess of S. Russia, I endeavoured to discuss its character and origin, and to compare the facts observed with the results obtained by many students of the latest phases of our earth's history. In so doing I overlooked two articles on this subject, which appeared in the Geological Magazine for 1882, and embodied the views held by Baron von Richthofen and Sir H. Howorth respectively. These may be briefly considered ere we turn to a special examination of the structure and relations of the Black Earth. An examination of Sir H. Howorth's statements reveals serious points of disagreement between the theories previously or subsequently suggested, and those which he himself holds. At the outset (*loc. cit.* p. 12) Sir Henry remarks: "Loess is not found in S. Russia, nor yet in the flat country bordering." Such a proposition would be deeply resented by the geologists of that country, and has unconsciously on my part received its answer in my previous paper, where I have shown Loess to be one of the most striking deposits of some of its southern governments, and that it presents all the most important features mentioned as characteristic by Baron von Richthofen.

But Sir Henry goes further, and having successively attempted to crush the various views held on this subject, propounds a theory of his own, which may be considered in the light of the results obtained from the particular district under discussion. The proposition practically may be stated thus: The Loess is a subterranean or volcanic product, which has been subsequently distributed by diluvial action.

This suggestion is also extended to the Black Earth, which, it is stated, is distributed about an apparently recently disturbed mountain focus, viz. the Southern Urals. So far is this from being the case that the Kharkov government, where some of the finest Black Earth districts exist, is 1000 miles from the above mountains, and over 500 miles from the Carpathian range. In reality the country is practically undisturbed, the Tertiary and Cretaceous beds having so small a dip, that the slight rolling of the strata is only observable when the conditions along a line of over fifty miles are passed in review.

Further, there are no traces of volcanic action in any of these central districts. The nearest exposures of eruptive rocks are probably the Carboniferous andesites and rhyolites of the south of the Ekaterinoslav government, the diabases (probably very old) in the W. of the same province, and the basalt mass rising through the Chalk of Central Poland.

Finally, if the Loess were of volcanic origin, it would contain traces of minerals, and possibly of structures produced under such conditions. Such evidence is not only wanting, but is *à priori* im-

[1] For No. I. Cretaceous, see Geol. Mag. September, 1892; No. II. the Loess, see Geol. Mag. December, 1892.

probable. Under these circumstances the prime cause operating, according to Sir H. Howorth, in the production of Loess and Black Earth appears entirely inadequate. At the same time, Sir Henry is to be thanked for the number of facts which he has brought together after undoubtedly long and careful reading, though the interpretations he has put upon them may frequently be open to much adverse criticism.

The second theory, that of Baron von Richthofen, is set forth clearly in the same volume as the above (pp. 293–305), and further study of his views, there brought together in a small compass, only serves to strengthen my belief in the great value of his contribution to this subject.

It is not necessary here to repeat how completely the vast majority of the conclusions formulated by him with regard to the Loess have been proved to be applicable to the sandy clay of S. Russia. Further, wherever we find unbroken land-shells, and remains of a typical steppe fauna, there the presence of Loess will have, in all probability, to be attributed to the action of wind.

Baron von Richthofen points out that there are two great classes of places where the dust of continents will rest permanently, and continue to accumulate through ages:

"The first includes the central regions of continents, where the water has no drainage towards the sea, but is collected in inland basins, from whence it gradually disappears by evaporation." (For further details see p. 298.)

"The second comprises such regions as the prairies, savannas, llanos, pampas, and *steppes*, where there is an alternation of a dry and a rainy season. Where this occurs the amount of dust put in motion and distributed through atmospheric agency can reach enormous proportions." Such conditions are in a measure prevalent in S. Russia, where a very dry summer frequently succeeds the long winter snows, and an autumnal rainy period follows the summer droughts.

As regards the original glacial origin of the Loess advocated by me, Baron von Richthofen fully agrees in regarding it as partly resulting from glacial trituration. Indeed, he came to the conclusion ("China," vol. ii. p. 748) that the time of the deposition of the Loess as a steppe formation in Central Asia coincides with the Glacial era in Europe, and in Germany it appears to have been formed after the first, and before (or during) the second glaciation.

Practically, then, the differences between the views held by Baron von Richthofen, and those which I have been led to adopt, are reduced to a minimum. Indeed, one cannot but be struck with the keen appreciation of every point of importance shown by him in his classical treatises. In my previous paper I failed to comprehend how fully he had discussed the subject in its various bearings, and desire here to tender an apology for my unwitting misrepresentation.

Subsequent to the publication of my paper in the Geol. Mag. December, 1892, Prof. Dokoutchaieff of St. Petersburg published a note in the Bulletin Soc. Géol. Belge, January, 1893, on the Loess

of S. Russia, in which he rejects the æolian theory, and advocates that of glacial origin.

He considers the deposit under several heads:

1. The Loess may be of glacial, marine, lacustro-fluviatile, eluvial, or diluvial origin.

2. The fine-grained Loess of the Poltava or Nijni-Novgorod type, of uniform structure throughout its thickness (except where a few small erratic blocks are present in its lower portions), and containing terrestrial or marsh-living organisms only; this Loess, covering the watersheds and ancient slopes, *is of exclusively glacial origin*, and represents the finer constituents of the glacial mud deposited over a country already covered with vegetation, and inhabited by the typical rodents of the steppes (as the *Alactaga*, the *Spermophilus*, the *Arctomys*, and the *Lagomys*); by the Mammoth and Rhinoceros, and occasionally by the Beaver.

3. A similar Loess occurs sporadically as little islands throughout the whole of the districts bearing traces of glaciation, sometimes in the inferior sands containing erratic blocks, and sometimes in the northern brown clays; but they become less frequent as we approach the sources of the Russo-Scandinavian glacier.

The Glacial Loess occurs principally (1) at the S. and S.E. limits of the glacier itself, and (2) in that part of the open country immediately contiguous to its probable boundary. In the first case it may cover all the watersheds; in the second it will only be in those districts where the absolute height is inferior to that of the most elevated points of the neighbourhood occupied by deposits of erratic blocks. For example, in the government of Poltava the maximum height of these deposits above sea-level is 210 metres, consequently in the whole of the lower basin of the Dnieper it should only occur at heights less than 210 metres.

4. One of the most characteristic peculiarities of the Glacial Loess is the presence of the dark-grey glacial mud, forming a definite but interrupted layer, frequently attaining a thickness of over two metres. It is distributed in patches, containing many large grains of quartz, and sometimes calcareous inclusions. The humus percentage is 3.

5. In the eastern wing of the glacier, that of the Don, the Loess is a stratified, coarse, and somewhat porous clay, of a brown-grey colour, but its origin is similar to that previously mentioned.

6. The typical Loess is absent, and cannot in general exist on the very ancient portions of the earth's crust. Exceptions occur along the whole of the Dnieper Valley, at the exterior limits of the glacier, and on the littoral of the Sea of Azov, where marine are interstratified with glacial deposits; Loess possessing the type characteristics also occurs in the south of Poland.

7. The Asiatic Loess is in all probability partly glacial in origin, but may be also eluvial, diluvial, or alluvial; only in isolated regions, occupying ravines of greater or less depth, will it be distinctly of æolian derivation. Prof. Dokoutchaieff would also remove the Loess of W. Europe from the æolian category.

8. The diluvial deposits of the government of Poltava are divisible into three horizons: (1) A fresh-water marl at the base; (2) Red clay; (3) Loess; all these, however, passing gradually into one another, whether in respect of organic remains, stratigraphical position, or lithological character. There is no evidence especially favourable to the conception of more than one Glacial epoch.

9. Seeing then that the typical Loess is, in the government of Poltava, restricted to the region of inferior morainic deposits, and only extends beyond them to a slight extent; viewing the fact that it is completely analogous to glacial mud occurring in purely morainic deposits, erratic blocks also being sometimes found at its base; bearing in mind also that Loess may be found below the brown morainic clays—*the Loess must be considered as a glacial mud deposited from glacial waters.*

This valuable contribution to the discussion of the origin of Loess comes from the pen of a distinguished geologist, who possesses a wide knowledge of the superficial deposits of Central Russia and the Poltava government. It seems to be the case that the theory adopted by an observer depends very much on the district in which he first studies its characters. Those who examine it along the boundary of the old Russo-Scandinavian glacier, a boundary indicated by morainic clays containing rocks of Finnish origin, are at once led to regard the Loess as a product of glacial activities. It immediately overlies the Boulder-clay, is stratified at the base, and clings closely to the ancient line of ice-action. Others, travelling over the southern steppe-land, would be struck by the tremendous activity of the varying wind-currents, giving rise to innumerable dust-clouds, and would not fail (now that Baron von Richthofen has called attention to the importance of these phenomena) to regard these remarkable deposits as resulting from æolian action, a conclusion which would be greatly strengthened by the discovery of perfect terrestrial mollusca, and the presence of a true steppe fauna.

As to the glacial origin of some types of Loess, most students of the subject are now in agreement, and it has, as above stated, been conceded by Baron von Richthofen himself, that such may be the case.

But it is by no means restricted to the boundary of the glaciated area. It extends all along the right bank of the Dnieper, and is said to be very marked on the banks of the Don. It appears, therefore, that when the general drainage directions of the country were first marked out, the flooded waters carried the fine glacial silt with them, and it was only on the establishment of conditions similar to those now prevailing that the rivers, confined to narrower channels, were engaged in denuding the deposit which their ancestral streams had assisted to transport.

Whilst on many points we are in full agreement, Professor Dokoutchaieff scarcely does justice to the vast importance of æolian activities operating during the many years that have elapsed since steppe-conditions came into existence.

After a reconsideration of the literature of the subject, I find no

reason to modify the propositions submitted more than a year ago (GEOL. MAG. December, 1892). These are: I. That the Russian Loess was originally the fine mud derived by the triturating action of the ancient glacier. This view has been still more strongly urged by Prof. Dokoutchaieff. II. That it was deposited by slowly-moving waters (this would embrace the aspect adopted by Prof. Armachevsky), and was distributed by rivers in flood. This latter action would account for the great southward extension of the Loess along the valleys of the Dnieper and the Don, and would by inference suggest that the general courses of these two great streams were already marked out. The thick deposits in the Danube and Rhine valleys may be explained in the same way. Here, let me say, seems to be the time for considering that mighty diluvial change, the Deluge, which has left its mark on the old historical records and traditions of so many nations. Sir H. Howorth weakens his argument by introducing prime causes, which, it is to be feared, he will have great difficulty in proving; but as one deeply interested in modern as well as ancient history, I should be delighted if he could bring our geological data and the monumental references into perfect accord. Only by a frank acceptance of a Glacial period in some limited form, will he be able to attack the serried ranks of the Uniformitarians with some hope of success, but combining the results due to its melting, and the changes in the mountain-structure of E. Europe, he may yet hope to carry on a not unsuccessful warfare. III. The glacial and diluvial actions were mainly restricted to the earlier periods of post-Glacial history, and determined the general geographical relationships of the Loess; but it was not long ere the wind became the redistributive factor, and, acting through long years, established those numerous characteristic features which can only be explained by means of an æolian theory of origin.

Having now briefly reconsidered some of the theories with regard to the origin of the Loess, let us now turn to the study of the Black Earth as it is developed in S. Russia. It has long attracted the attention of geological observers, not only on account of its striking appearance, but also because of its rich fertility as a soil. Its wide distribution has been specially noted by Sir R. Murchison ("Russia and the Urals," p. 557), where he says: "Its N. limit is a waving line, passing from near Kieff to Tchernigov, a little to the S. of Lichvin along the 54° latitude line, then advances E. to 57° N. latitude, and occupies the left bank of the Volga between Nijni Novgorod and Kazan. It is also abundant on the Kama and Ufa." Its wide occurrence in Siberia is then noted, as also its presence on plateaux more than 1000 feet above sea-level in the Bashkir country, and on both flanks of the S. Urals. "It does not occur much in the space occupied by the Aralo-Caspian beds, nor is it found S. of the granitic axis. It lies on rocks of all ages and is found at very different levels. The northern drift is succeeded, if not actually overlapped, by the Black Earth." It has, in reality, a far greater western extension than has been ascribed to it in the above lines, and I

have noticed it not only in the Jitomir district, about a hundred miles to the W. of Kieff, but also in passing from Russia into Austrian Poland. The last locality at which it was actually observed by me was Jaroslavl, a town situated on the railway between Cracow and the old city of Premysl. Beyond that point Loess alone seemed to be the prevalent surface deposit. The first point of importance is the close connection existing between the Loess and the Black Earth. Murchison included both deposits under the common term Tchernozem; but this latter, in the truest sense of the term, should be restricted to the upper strata, rich in humus contents. Schmidt regards it as practically identical with Loess (Zeits. Deutsch. Geol. Ges. vol. xxix. p. 830–31). Richthofen states: "The black colour, which is proper to the uppermost layer only, appears to result solely from the formation of vegetable mould, the deeper portions showing the brown colour of the Loess, together with its structure, although this appears to be less perfect than in the former case." Personal observation has left no shadow of a doubt on my mind as to the intimate connection between these two types of deposit. Reference has already been made as to the gradual passage from pure Loess to Black Earth observed in a ravine at Kharkov, and an example of the interlamination of the two at their point of junction has been cited from Bielgorod.

But these cases, developed on a small scale at the above localities, may attain much greater proportions. Repeatedly the dark brown humus-bearing bed forms distinct lenticular patches (often 3 or 4 feet in thickness at the centre and extending some little distance in a horizontal direction) running through the yellow clay.

The further and final stage is attained when the Black Earth forms a distinctly marked stratum, actually overlain by a true Loess. This is beautifully observed in the ravine near the railway station at Kieff, where a thick layer of Black Earth is overlain by some seven feet of Loess.

But the inter-relationships of the two deposits are still further brought out if a closer examination be made of the materials composing them. Not only does Black Earth agree with Loess in its superficial position, its existence at most varying level, its occasionally purely local development, but it possesses the same want of consistency when rubbed in the fingers. The chemical composition is in most respects similar. Thus Loess may be said to be composed of pure silica in the form of sand-grains, clays, calcareous constituents (as concretions frequently), and oxide of iron, acting as a colouring material. Compare with this the analysis of Black Earth made by Mr. H. Phillips for Murchison's work:

Silica	69·8
Alumina	13·5
Lime	1·6
Oxide of Iron	0·7
Organic Matter	6·4
Traces of Organic Matter H_2SO_4, Cl., etc. ...	1·7

The amount of silica is always very noticeable. The high percentage is due to the large number of polished sand-grains always present in samples of Tchernozem. These, however, vary greatly in size, from the grains $\frac{1}{18}$ of an inch in diameter already mentioned from Bielgorod, down to particles almost invisible to the naked eye. The variations appear in some respects, to follow the rule, that the further south the deposit the smaller the sand-grains contained in it. It would require a close examination of a large number of samples before this could be established as an absolutely proved fact.

In doing so, particular care must be taken to select the specimen from an uncultivated spot. This is now a somewhat difficult task, so much of the steppe-land having been brought under cultivation. Examination and enquiry have both, however, led to the same result—the deeper the earth from the surface the more sand-grains does it contain. Both in character, position, chemical analysis, and history, the Black Earth is thus intimately connected with the Loess, and is due to special circumstances affecting the latter.

A historical review of the whole subject is of particular interest, as showing the various possibilities which have been brought forward to account for these soils, the treeless prairies and steppes, and the sharp distinctions between present geographical conditions in N. and S. Russia. At the same time a remarkable parallelism is to be observed between the theories propounded in America, and those which have from time to time been enunciated by Russian geologists. In the Journ. Poltava Agricult. Soc. February–May, 1891, Prof. Krasnov has given an admirable summary of the latest position of these discussions as viewed from Russian standpoints. Many of his conclusions have struck me as being of such general interest that it may be advisable to include them in the present discussion, especially as they are at present practically buried in a local Russian journal. Pallas was one of the first to consider the origin of the Tchernozem. He, in common with later observers, regarded it as having been formed on the bed of a gradually-dying sea, the water-plants and other organisms of which, rotting in contact with the atmosphere, formed its mass, and gave rise to its richness in nitrogeneous products.

Murchison (*loc. cit.*) considered it as not having been formed *in situ*, but derived from the disintegration of the Black Jurassic shale, which is found in the more northern districts of the empire. He held that powerful currents had acted as the transporting agents, though no reason was assigned for the production of this rush of tumultuous waters. At the present day ice-action explains all these varied results in a more reasonable manner.

Ruprecht seems to have gone half-way in regarding the Black Earth as of grassy origin, though he still held the view that the boulders strewing the northern parts of the country had been brought thither by ice-bergs.

But, with the acceptance of the theory propounded by Nikitin, Krapotkine, and Inostranzeff, that a great fresh-water sea did not cover N. Russia, but rather that a huge ice-sheet spread over the face of the country, a new aspect was obviously given to the discussion, and it was sought to explain the phenomena by other means.

The former presence of forests was naturally regarded as necessary to account for the formation of such a soil, if all aqueous action were rigidly excluded. Thus, Bogdanoff and Polimpsest considered the Black Earth to be the humus resulting from prehistoric woods, cut down by hordes of nomads. For the extensive character of such forests they sought to find evidence in the works of Herodotus.

In his work Melpomene, Book IV., Herodotus gives some very concise descriptions of the nature of the country lying between the Dnieper (Borysthenes) and Don (Tanais). He appears, indeed, to have made this region an object of special research. For instance, it is expressly stated that the Androphages (inhabiting Volhynia) lived northward of a region of vast deserts (the great Loess-covered plains east of the Dnieper). In another place it is stated that the entire country occupied by the nomadic Scythians is without trees. (This is evidently the district we are specially considering.) And this is still more forcibly emphasized when considering the country N. of the Azov Sea. The country of the Sauromates commences at the extremity of the Palus Mœotis, and occupies the country that lies to the north ; a fifteen days' journey is required to traverse it; *one sees there neither wild nor fruit-bearing trees.* Beyond this is the country of the Budins, where trees are in abundance.

It is also interesting to note that to the north of the country between the Dnieper and Don, only marshes and land without inhabitants were believed to exist.

Far, therefore, from proving the existence of former forests, Herodotus strongly argues in favour of the desert character of these regions, and indeed further cites Scythian traditions which fully confirm the view that this country was treeless ever since the earliest historical records were written.

It is necessary to emphasize these points, because some support might be lent to the opposite view, if the remarkable statements made by Mr. Floyer regarding the former fertility of many of the Egyptian wadys be considered.

The discussion in America has run to a large extent on absolutely parallel lines, and Mr. J. W. Rodway (Geographical Journal, March, 1894) has shortly resumed the views propounded, and arrived at a conclusion differing in no respect from the one now enunciated, viz. the areas in question never were timbered.

Mr. Miller Christy, in a paper read before the British Association, held that the absence of the trees was due to prairie fires, in general started by Indian tribes. But, as Mr. Rodway points out, these fires only affect the stems and branches, leaving the roots of the trees entirely untouched. That these splendid plains have not borne these growths for many centuries past, is shown by the fact, long ago observed by Murchison, that traces of root-fibres are altogether absent.

Prof. Krasnov also argues that woods do not form Tchernozem. On the contrary, they form a subsoil in which the component parts are of the size of nuts, of an ashy-grey colour, and poorer in humus than the Black Earth proper. In addition, this soil bears traces of

the roots of trees, at the same time possessing considerable powers of resistance to pressure, that is, failing to crumble readily when rubbed between the fingers.

Researches in the Nijni Novgorod and Poltava governments prove that a hundred years after the cutting down of the forests the soil still preserves its original character. If woods be not the source of the rich deposit, the present character of the steppe areas may serve to furnish a solution to the problem. There are still some districts not yet touched by the agriculturist, where the virgin soil produces a rank vegetation, luxuriant or sparse according to the amount of rainfall. A notable growth is a peculiarly wiry grass, that blunts the edge of the sharpest scythe. On the great plains E. of the Dnieper, in the region not devoted to the cultivation of cereals, Euphorbiaceous plants and Compositæ largely predominate. Indeed, so favourable is the soil for the growth of the latter, that one of the Thistles, *Xanthium spinosum*, (introduced some thirty years ago with the Bohemian settlers) has become a widespreading pest, and threatens even to supplant the indigenous flora.

In travelling over these plains, I early came to the conclusion that the decomposition of these plant-remains, and the intimate intermixture of a portion of their protoplasmic contents with the particles of the Loess soil, were the true causes of the formation of Black Earth. In the autumn the dried stems which roll over the steppe, often bound together in large bundles, bear witness to the large amount of plant-waste which must then go on. At the same time the roots remain in the soil, giving rise to great matted fibrous layers which prevent the penetration of water to the lower levels. Indeed, there are circumstances in which such roots may attain the thickness of a man's thumb. Many of them will probably undergo decomposition, but, thanks to the fine-grained character of the soil, the materials necessary for the production of the humus would be concentrated rather than diffused through it.

Von Richthofen (*loc. cit.* p. 300) remarks: "The Black Earth of Southern Russia is growing (owing to dust transported by wind action). The black colour, which is proper to the uppermost layer only, appears to result solely from the formation of vegetable mould, the deeper portions showing the brown colour of the Loess, together with its structure, although this appears to be less perfect than in the former case."

Prof. Lewakowsky, of Kharkov University, has approached this interesting subject from the chemical point of view. He finds that the organic matters form, at a shallow depth, definite chemical combinations with the soil constituents, alumina, iron oxides, and calcium compounds. These compounds are exposed to the oxydizing action of the air, becoming gradually of darker tints, till eventually they form the Tchernozem itself. He explains the absence of Black Earth in forest regions as follows: The soluble organic materials which are prevented from passing into the soil, owing to the thick deposit of fallen leaves, undergo changes under the influence of the atmosphere, and become insoluble or even burnt by its oxydizing

action. I would suggest that in all probability, even if these materials did sink into the ground, its loose constitution would prevent the formation of any deposit as rich as Black Earth. Owing to the matting of the roots above-mentioned, it is always necessary to remove the upper layer of black soil round any spot where trees are to be planted. Mr. Christy has maintained that trees on the treeless plains grow readily. Mr. Rodway admits this to be true in part. For Russia, I may venture to go further, and say that Black Earth is in many respects absolutely inimical to the growth of wood or forest.

(*To be continued in our next Number.*)

VII.—Remarks on Mr. Mellard Reade's Article "On the Results of Unsymmetrical Cooling and Redistribution of Temperature in a Shrinking Globe as applied to the Origin of Mountain Ranges."

By A. Vaughan, B.A., B.Sc.

[*Addendum to paper on Corrugations of Earth's Surface.*]

AFTER I had written the above dissertation and sent it for publication my attention was kindly drawn to a paper, dealing with the same subject, communicated by Mr. T. Mellard Reade, C.E., F.G.S., to the Liverpool Physical Society, and since republished in the May number of the Geol. Mag. for this year (p. 203).

As I was not aware, when engaged upon my own paper, of the views which Mr. Reade had expressed, I feel that some discussion of the theory put forward by that writer is of necessity demanded. Since, however, Mr. Reade's views are extremely antagonistic to those which I have already endeavoured to explain, such discussion will find its most convenient place as a brief addendum to what I have already written.

Mr. Reade's paper opens with a statement of some results arrived at by Lord Kelvin in his classical paper on the "Cooling of the Earth"; since the utmost importance is necessarily attached to the conclusions arrived at by so eminent a physicist, it is very important that such results should be quoted correctly, and also that their statement should be accompanied by a short exposition of the assumptions on which they are based. I propose then, in the first place, to point out, as plainly and briefly as possible, the method of reasoning employed and the data assumed in Lord Kelvin's essay; this I regard as especially necessary, because Mr. Reade not only mis-states some of the most important results, but also invests them with a rigidity to which the writer expressly lays no claim.

The essence of the employment of Fourier's equations depends upon assuming that we are dealing with nothing but the *conduction* of heat from point to point. Thus, no heat must be supposed to be used up or evolved in changing the physical state of the material under consideration; for example, we cannot suppose that any large portion of the mass has, during the time we are considering,

passed from the molten to the solid state; for this would involve the liberation of that amount of heat which would be necessary to convert the solid rock into a molten state at the same temperature. If applied to any period during which such changes of physical state took place our equations of conduction would simply be talking nonsense; so that the first important point assumed by Lord Kelvin is that, during the whole period to which he applies his reasoning, the mass under consideration is supposed to be in the same physical state, and therefore, of course, at all times solid.

Another point to be especially noticed is that there is supposed to be nothing in the nature of discontinuity throughout the mass, or, in other words, that heat is freely conducted from layer to layer. In consequence, any portion of the mass exhibiting such phenomena would not satisfy the only condition on which our equations can be used; so that when applied to such portions our equations cannot be regarded as telling us the absolute truth. Now, as far as the interior of the earth is concerned, we can no doubt assume perfect continuity with sufficient exactness; but when we approach the surface we come upon very heterogeneous rocks, separated by bedding and other separation planes, so that we introduce an element of discontinuity.

This new element will not, to any appreciable extent, affect the general considerations put forward by Lord Kelvin, but will, on the other hand, render results deduced from our equations in the neighbourhood of the surface necessarily fallacious.

I attach some considerable importance to this condition as tending to vitiate the practical value of results concerning a level of no-strain to which I shall refer later, and also as exerting a conservative influence in the matter of the outflow of heat, and, in consequence, as tending to lengthen the time from first consolidation.

Lastly, for purposes of calculation we require to know a certain average value of the coefficient of thermometric conductivity, and this involves the two considerations of conductivity and specific heat. Of the difficulties attending such a determination, it is only necessary to point out that the variations of these two physical quantities with pressure and temperature are as yet very imperfectly known, and that, consequently, the assumption of any such coefficient for the unknown rocks of the interior must necessarily be an extremely tentative one. But, supposing such an average coefficient to have been found, it obviously would not agree at all closely with that for rocks near the surface; for, in its very character as an average coefficient, it must essentially refer to the vast bulk of the interior rocks, and not to the comparatively insignificant crust.

Thus we see another reason why, though our equations may give us exact results with regard to the interior, they cannot, for that very reason, apply rigidly to rocks near the surface. In fine we are not justified in applying Fourier's equations of conduction to rocks near the surface, or, at all events, not equations involving the same constant of conductivity as we have used for the interior.

In the next place it is important to notice the data which are assumed. Lord Kelvin assumes that, at the epoch of consolidation, the earth, or at least an outer shell of a thickness of 100 or 200 miles, was at an uniform temperature, and that the temperature of the surface was subjected to a loss of 7000° F. and then maintained at this lower temperature.

The temperature at the very start was, in consequence, uniform downwards, not a rise of 1° F. in 10 feet as, by some very curious oversight, is stated by Mr. Reade. This latter temperature gradient, which is given as that of first consolidation, was not established, on Lord Kelvin's assumptions, until 4,000,000 years later.

In this reduction of 7000° F. at the surface we have, at the best, a probable hypothesis with no pretence at exactness. The value is, of course, mainly based upon the heat set free when the earth, or a very thick outer shell, passed from the molten to the solid condition; in other words, upon the latent heat of fusion for the earth rocks.

Starting from this datum, and assuming the applicability of Fourier's equations, the temperature which at any given time existed at a given distance below the surface is easily calculated, and it is found that, after 100,000,000 years (not 96,000,000 as quoted by Mr. Reade), the temperature gradient would be 1° F. in 50 feet, which Lord Kelvin assumes as the present temperature gradient very near the surface. Since, also, this gradient varies inversely as the square root of the time from consolidation, it is easy to obtain the number of years which must have elapsed to establish any particular rate of rise of temperature with depth. For example, for a gradient of 1° in 64 feet, the time would be 160,000,000 years for the same initial reduction, and the depth to which loss of temperature would have penetrated would be 180 miles, as opposed to 150 miles on the assumption of 1° in 50.

To emphasize how broadly these results must be looked at, it is only necessary to point out that doubling the initial reduction of surface temperature would multiply the number of years before the establishment of any stated gradient by four, and that halving the coefficient of conductivity would double the time. For example, if the initial reduction were 10,000° F. the number of years would amount to about 300,000,000. This is in accordance with the wide general limits assigned by Lord Kelvin in his essay, in which he places the time from first consolidation as somewhere between 20,000,000 and 400,000,000 years.

Again, owing to the partial inapplicability of our equations to surface rocks, it is questionable whether we can, with justice, assume that 1° in 50 feet expresses the surface gradient which would have been established were the whole globe continuous. In short, the effect of discontinuity in surface rocks would seem to point to a retardation in the loss of heat and a consequent increase in time from consolidation.

To pass on now to consider the value of the assumption of a shell of no-strain: The contraction taking place in one year at any point

within the earth, if unresisted, would no doubt be proportional to the loss of temperature at that point in that time, and this, assuming the truth of our old equations, would be proportional to the rate of increase of the temperature gradient at the point. We thus obtain the simple conception that the contraction will increase from the surface inwards to a certain depth and then diminish, until the point is reached at which temperature becomes uniform downwards.

The distance of such a shell of maximum contraction below the surface, estimated in the way already indicated, will be equal to $\sqrt{kt}$, where k is the coefficient of thermometric conductivity, and t the time from consolidation in years. This gives, assuming the same data as before, a depth of 38 miles. (Mr. Reade states that the value is 50, which would be the depth after about 160,000,000 years.)

From the fact of the existence of such a maximum-contracting shell, Mr. Reade has deduced with perfect theoretical correctness, so far as a homogeneous sphere is concerned, the necessary existence of a level of no-strain. In other words, there should, theoretically, be a shell, between the surface and this shell of maximum-contraction, which could contract with loss of temperature so as not to exert any pressure on the underlying mass; whilst all shells below it would, in contracting, squeeze the interior, and all shells above it would, owing to less rapid contraction, attempt to stand off from lower shells, and would, in consequence of the force of gravity, be put into a state of compression.

It becomes then a very important problem to find the depth below the surface at which this level of no-strain lies; and this appears to be easily attacked in the following manner.

Choosing any shell at a given distance from the centre of the earth, we can easily find, by the foregoing analysis, the radial contraction which has taken place in one year in any of the shells below it which suffer loss of temperature, and, by summing all these separate contractions, we obtain the whole amount by which the interior has attempted to draw itself away from our chosen shell in that time. If we equate this amount to what would be the radial contraction in one year of a sphere with radius equal to the distance of the selected shell from the centre, and further suppose the whole of this sphere to be contracting at the same rate as the given shell, we shall find the distance of such a shell from the surface. Working this problem out with our preceding assumptions I obtain a distance of a little over a mile from the surface. I have felt it necessary to give the reasoning employed, as this result differs from that found by Professor Darwin, as quoted in Mr. Reade's paper, and to whose high authority I should naturally attach great weight; but I have not, unfortunately, had the opportunity of referring to his paper.

Assuming, however, that Professor Darwin is correct in estimating the depth of this shell of no-strain at two miles below the surface, I cannot think that the result has any but a theoretical interest.

For here we arrive at a point to which, as I have shown above, the method of analysis cannot rigidly apply, not only on account of the physical discontinuity of surface rocks, but also on account of the irregular distribution of the large land and ocean areas which renders a shell at the depth of two or three miles a purely theoretical conception.

In fine I adhere to the conception, so fully explained in the beginning of my paper, that, owing to the existence of separation planes, this imaginary shell of compression settles down upon the interior rocks; so that in reality we are dealing with a vast series of shells, contracting upon and consequently squeezing each other, surrounded by a thin layer of surface rocks, which settles down upon them by closer application.

Before criticizing the theory put forward by Mr. Reade, I should like to point out the questionable strength of his disproof of the older theory.

Assuming that contraction has only affected a shell about 150 miles in thickness, he supposed this shell to have lost an average of 1000° F. since Cambrian times, and proceeds to calculate its radial contraction.

To quote his own words:—

"The linear contraction of 150 miles of rock cooled 1000°, using this coefficient, would be 4,125 feet, but, as the contraction of the shell 150 miles deep is voluminal, the contraction in thickness of the shell would be three times this, or 12,375 feet or=2·344 miles."

I fail to see how the contraction in thickness, *i.e.* the contraction of a certain line of material, can by any possibility be voluminal. But, accepting this result for the sake of argument, Mr. Reade finds that the girth of the interior shrinking sphere would be 15 miles less than that of the thin imaginary outer shell of compression; so that this represents, so to speak, the amount of looseness which has been used up in mountain-making since Cambrian times.

Here Mr. Reade leaves this part of his argument, but if we apply the above result we shall find that the elevation which could thus be produced is a very large one indeed. For suppose, to take a case far from the maximum possible, that the extra length of 15 miles in each great circle is used up in forming a cone tangential to the interior sphere. We should thus be able to form a mountain covering nearly a million square miles and rising to a height of 60 miles above the surface of the sphere. But, as Mr. Reade says, his assumptions are generous in the extreme, and I feel sure that, as referred to above, he has gratuitously multiplied them by three.

In the very simple calculation presented early in this paper. I took a coefficient not differing much from the one adopted by Mr. Reade, and made allowance for a loss of 10° C., which I believe would more than cover any one period of mountain-making. By this line of reasoning we seem to obtain a better means of comparison with actually known facts than if we estimate the total available volume which the old theory would allow for mountain-making since Cambrian times, and then attempt to compare it

with the very unknown volume which has, in reality, been employed in constructing all the mountains since that period.

I now wish to examine Mr. Reade's theory, and to compare it with that put forward in this paper.

In the first place, Mr. Reade imagines a great depth of sediment to be laid down over a large area, and, to fix ideas, suggests an average depth of 4 miles of sediment deposited over an area 2000 miles by 1000 miles.

No doubt it follows, with perfect accuracy, that, as the layers accumulate with great slowness, the temperature gradient at any time during deposition will simultaneously attain its average value, and be roughly, say, 1° F. in 50 feet downwards; so that, as any layer becomes buried deeper and deeper beneath successive deposits, its temperature will continually rise.

For the same reasons, the rocks which constitute the floor upon which sediment was first deposited will suffer a continual increase of temperature. After a certain accumulation of sediment, the expansion produced by this rise of temperature will cause the underlying rocks to curve upwards, so that the curvature of any layer, which was at first approximately that of the surface of the earth, becomes increased and allows of expansion, much in the same way that a bar of iron, bent into an arc and fixed at each end, will, if heated in the middle, bend into a steeper curve.

I have taken great care to state exactly what I understand to be Mr. Reade's views, in order that my criticism may be comprehended better. I may, perhaps, just point out that this extension has, of course, the effect of a vertical lift upon the overlying mass, and that it is to this vertical motion that Mr. Reade attributes the inception of mountain ranges.

Now, it is a fair deduction from the very slow rate at which sediment accumulates, that the full effect of extension proper to any given depth of sediment will have been brought about at the time at which that depth is established. Such a process would necessarily go on until the central portion of the region was raised to the sea-level and the deposition of sediment in consequence ceased, and no further rise could take place, at least so far as due solely to expansion of underlying rocks.

The mechanism by which the fold thus formed is further compressed and raised by lateral pressure, I fail to understand; especially in the light of Mr. Reade's insistence that expansion is internal and shading off to zero at the boundaries of an area of deposition, so that any new area of sedimentation which may be established on the sides of the old fold would produce a new central fold, and not exert lateral compression on the earlier one. It is also hard to see how such an area of deposition could exist outside the first fold, except as derived by marine denudation of that fold itself.

Again, the conception that any fold is formed must necessarily imply that the rocks which form it are drawn away from the underlying mass; for it is upon the amount of lateral expansion, and not upon the comparatively small vertical expansion, that the theory

rests. Hence it follows that an absolute vacuity must be formed under the fold, since in no way could a shell of expanding material exert a squeezing force on the rocks beneath. Thus the undermost of the layers which constitute the fold must support the whole weight of the overlying mass. This result does not seem to accord well with the potent effects which are attributed to the weight of overlying rocks in the direction of causing, what Mr. Reade calls, compressive extension. But, in fact, I believe that there is an element of unreality which renders the whole conception invalid.

In the first place, so far as the effect of heat upon the actual sediment is concerned, this sediment is deposited, not as solid rock, every component particle of which is in closest union with its fellows, but rather as a great number of small particles, each of which has, so to speak, plenty of elbow room. It would seem then that the effects of increase in pressure and temperature would be mostly used up in welding the rock into a dense mass; each particle expanding into the interspaces surrounding it, and the greater the resistance to general expansion the more closely would each particle be forced into union with its neighbours. This, surely, is in agreement with the actual facts observed in the lowest of a great series of conformable sedimentary strata. But, as regards the actual floor and the rocks which lie beneath it, which may be supposed to be in a dense state and not to admit of any great expansion of separate particles, I believe that the effect of expansion will only tend to minimize the contraction which, I now attempt to show, must be in progress.

It will, assuredly, be readily granted that, to allow of the accumulation of the great thickness of sediment here conceived, there must be an approximately proportional depression of the floor, and such depression must imply the curving inwards of that zone of the earth which forms the area of depression. Such inward motion must result in the reduction of curvature, and the consequent diminution of area; in other words, the floor must be contracting and must continue doing so approximately as long as sedimentation proceeds. Thus the only effect of expansion, in such an area, must be in the direction of minimizing the rate of shrinking, and, should the effects of expansion balance those of contraction, the area ceases to sink and deposition soon ends.

I will now briefly point out what I conceive to be the true phenomena which cause and accompany the deposition of a great thickness of sediment near land.

Imagine an area, such as I have conceived in the statement of my theory, to be bordered by a large land region. Owing to the more rapid contraction of our depressed area, its floor bends inwards, or the area sinks; over that portion of the area which borders our supposed land region sediment will be deposited and will accumulate to a greater and greater thickness as the area sinks. This sinking must obviously be accompanied by the actual displacement of material from underneath, which will be forced below the surrounding regions, and increase their relative height. Thus, as long as

sinking continues, there is a transfer of material, squeezed out from under the depressed area, and forced below the bordering land region.

As a great thickness of sediment accumulates on the margins of the main area of depression (which is itself far from land), owing to the establishment of a temperature gradient throughout this sediment, the contraction of this border region becomes continually less and less than that of the main area of depression; and at last the region of sedimentation ceases to sink and begins to rise owing to the introduction of material, squeezed out from under the more rapidly contracting area.

To sum up, the sequence of events will be briefly as follows:—

First, when sediment is accumulating, the neighbouring land area is rising, owing to actual transference, from under the sinking area to beneath the neighbouring land region.

Secondly, this continues until, owing to the thickness of sediment accumulated, the area of deposition contracts at a much slower rate than the main area of depression, and, in consequence, is forced up by the introduction of material squeezed out from beneath that area. Thus the tendency is, as I stated above, to narrow the main area of depression.

The last point in Mr. Reade's paper, to which I wish to call attention, has reference to the effects of denudation.

As far as I am able to understand the very brief statement contained in his paper, Mr. Reade argues that, as foot after foot of rock is removed by denudation, the temperature of any point beneath is gradually lowered, so that contraction sets in, which doubtless tends to draw the rocks in such an area of denudation away from those surrounding it.

The actual results which Mr. Reade considers would follow are best given in his own words:—

"The weight of the crust itself squeezes up all vacuities, and the area adjusts itself to its decrease of volume by normal faulting and keying up in a wedge-like manner and by compressive extension."

Now to bring about normal faulting the rocks must not only split apart, which is the necessary result of contraction, but this must be accompanied by vertical displacement, and I can see no possible cause contained in the theory which accounts for the vertical sinking of one part of the area of denudation relative to another.

Again, as regards what Mr. Reade calls compressive extension, I think it is a sufficient answer to point out that contraction is greatest nearest the surface; for it is always there that the reduction of temperature must originate. In other words, the first layers to contract are the outer ones, or those under the least overlying weight; and further, in contracting, each layer must tend to assist the contraction of one lower. To bring to a close a paper perhaps already inexcusably long I will briefly recapitulate my own views on the results to be expected in such a contracting area of denudation.

In so far as contraction is general over the area, the tendency will be for the whole area to attempt to bend inwards, and the result of such a tendency is to check the introduction of material which,

as I have attempted to show, is being squeezed out from beneath the neighbouring area of depression. Thus, such material will be more and more used up in raising the bordering one of sedimentation, with the ultimate production of a parallel elevation.

In so far as contraction actually produces splitting, this will, over a region with no especial lines of weakness, produce a very complicated system of joints throughout the entire mass; and, by thus slowly relieving pressure, bring about the formation of holocrystalline igneous rocks beneath.

VIII.—How Chlorite is Converted into Biotite.

By C. Callaway, D.Sc., F.G.S.

GENERAL McMAHON'S paper in the Geological Magazine for June attacks my conclusion that the Malvern biotite is formed from chlorite, but leaves my evidence untouched. Indeed, a large part of the article is occupied in discussing a theory which I do not hold. I have never said that the chlorite was converted into biotite by contact-action *only*. That there may be no further mistake, let me repeat that, in the Malvern rocks, chlorite is changed to biotite by contact-action *plus* dynamic deformation.

General McMahon, in rejecting my theory, has rightly felt that he was bound to offer an alternative explanation. He is aware that the materials for the manufacture of the mica must come from somewhere, and he suggests that it may have been produced by "direct impregnation" from the granite. That the granite has impregnated the encasing diorite with one at least of its constituents, viz. potash, is one of my own points; but that it has bodily produced the biotite is simply impossible. This granite, it must be remembered, consists almost exclusively of quartz and potash felspar. An analysis of it by Mr. Player gives 1·1 per cent. of iron-oxides, and 0·3 per cent. of magnesia. Three analyses of the same rock by Mr. Timins yield about the same results, but one of them contains no magnesia. You cannot get a black mica without iron or magnesia, and it is certain that the granite could not have supplied enough of these bases for its manufacture. The encasing diorite, on the other hand, contains plenty of both bases.

General McMahon objects to my statement that the conversion of chlorite into biotite is an "observed fact." He writes that "the fact actually observed is the existence of chlorite and biotite in the same rock." So my critic believes that, merely because the two minerals lie side by side, I have inferred that the one was formed out of the other! I might as well have argued that the sugar in my tea-cup was evolved out of tea.

I will give General McMahon one or two facts, and allow him to judge whether they support his suggestion. Near the Wych, there is a very clear case of contact between diorite and granite. Both are sheared, and more or less altered. Near the contact, the hornblende of the diorite is decomposed; chlorite and iron-oxide being most conspicuous amongst the resulting products. Slides of the rock taken at the exact contact, show the decomposed diorite side by side

with crushed granite. The chlorite and iron-oxide are seen to be creeping in between the fragments of granite, and forming a matrix to them. Another slide, taken from within an inch, shows the same thing; but the signs of shearing, as distinguished from mere crushing, are evident. Many other slides, selected first at intervals of an inch or two, and then at greater distances, exhibit more and more clearly the laminated structure of the ordinary gneiss, as the shearing becomes more even and uniform. Thus we see produced a gneiss in which lenticles of quartz-felspar granite are interlaminated with chlorite and iron-oxide. Here and there patches of black mica appear in the chlorite, especially round particles of the opaque iron-oxide. In the same locality, at a little distance, the chlorite is largely replaced by the mica. The evidence for the origin of this mica appears to me irresistible. It must have come from either the infiltrated products, or from the granite, or in part from both. But it could not have come from the granite, for that contains a mere trace of iron-oxide and magnesia. It must, therefore, have originated from the chlorite and iron-oxide, with, no doubt, the assistance of potash from the partially decomposed granite.

But I will give my critic another case. I will put it in a semi-diagrammatic form, merely remarking that it represents what occurs in countless sections in the Malvern Hills. The distance from A to C is usually a few feet or yards.

A	B	C
Ordinary diorite.	The same diorite, growing progressively sheared towards C, the hornblende going into chlorite, etc.; the felspar becoming corroded and yielding quartz.	A granite-diorite plexus. Chlorite of the diorite largely replaced by biotite, its felspar often deeply corroded, quartz abundant in it.

The gradation between A and C is clearly seen both in the field and in microscopic slides, of which some hundreds bearing upon this point have been examined. It is apparent in the felspars, whose changes can be followed step by step. It is equally clear in the basic minerals, the hornblende decomposing into chlorite, iron-oxide, and sometimes epidote, and the chlorite becoming banded with biotite, and, within the complex, being largely replaced by it. It appears to me inevitable that either the biotite has been produced out of the hornblende through the intermediate form of chlorite, or the hornblende has been produced out of the biotite through the same intermediary. But if the latter alternative be accepted, how is it explained that an unsheared diorite is produced out of a sheared micaceous rock?

General McMahon will perhaps suggest that there must be a break somewhere; that the biotite is decomposed to chlorite, and the hornblende is an independent mineral. This view is negatived by the fact that the hornblende and the chlorite pass into each other. A more reasonable suggestion would be that both the hornblende

and the biotite are decomposed into chlorite, and that a break occurs somewhere in this intermediate chloritized rock. But this view labours under enormous difficulties. For:—

(1) There is no evidence of a break, either in the field or the microscope. The changes that take place, whether structural or mineral, proceed *gradatim*, and are similar in numberless sections.

(2) The mineral transformations proceed *pari passu* with the mechanical deformation; hornblende, for example, in the ordinary diorite changing to chlorite in the crushed and slightly sheared rock, the chlorite, in its turn, passing into biotite in the core of the shear-zones. The felspar also undergoes transformations corresponding to the degree of shearing.

(3) The uniform appearance of mica where the shearing is great and where the granite-veins are numerous, while it is nowhere seen where shearing and veins are absent, appears almost to demonstrate that these are the true causes of the generation of the mica. It would be strange, indeed, if such powerful causes produced no effect. That the mineral changes gradually increase as we approach the veined complex is quite inconsistent with the hypothesis of a break. If there be no break, it appears to me that my conclusion is demonstrated.

My excuse for again troubling the readers of this MAGAZINE on this subject is that it has a significance beyond the mere mineral change. The origin of the biotite virtually carries with it the whole theory of the metamorphism at Malvern.

REVIEWS.

I.—PALÆONTOLOGIA ARGENTINA. II. CONTRIBUTIONS TO A KNOWLEDGE OF THE FOSSIL VERTEBRATA OF ARGENTINA. By R. LYDEKKER. (Annales del Museo de la Plata.) La Plata, 1893. (Edited by Dr. F. P. MORENO, Director of the Museum.)

THIS beautifully printed and illustrated volume is the outcome of work done by Mr. Lydekker during a short visit to the Argentine Republic in the autumn of last year. The confusion that had arisen in the nomenclature of the extinct mammalian fauna of South America was very great, and it was desirable that someone well acquainted with the vertebrate palæontology of the Old World should have an opportunity of examining the material which had been the subject of so many papers, in some of which, it was evident, new genera and species had been founded with a plentiful lack of discretion. The announcement, therefore, that Mr. Lydekker was about to visit the La Plata Museum, where many of the types are preserved, was very welcome, and, in spite of the shortness of the time at his disposal, he has been able to make important additions to our knowledge of the fossil vertebrates of South America. The present volume contains three Memoirs:

(1) The Dinosaurs of Patagonia.
(2) Cetacean Skulls from Patagonia.
(3) A Study of Extinct Argentine Ungulates.

1.—This Memoir is of peculiar interest, since it contains a description of the first Dinosaurs recorded from South America. These are referred to three genera: *Titanosaurus*, *Argyrosaurus*, and *Microcœlus*, the first two of which belong to the Sauropodous group, while the third is of doubtful position. The remains ascribed to *Titanosaurus*, a genus originally founded on some caudal vertebræ from the Lameta beds (Lower Cretaceous) of India, are provisionally referred to two species, and tend to show that, in some respects, *e.g.* the biconvex first caudal vertebra, this form approached the Crocodilia more nearly than any other Dinosaur at present known. Its wide distribution, occurring as it does in India, England, and South America, is also notable, but the discovery of further remains may show that the relationship between the species from these various regions is by no means so close as it now appears. The second genus, *Argyrosaurus*, is new, and is founded on a beautifully preserved fore-limb, the possessor of which must have rivalled the largest known Dinosaurs in size. The third genus, *Microcœlus*, is also new and is represented only by a few isolated bones. A small dorsal vertebra is doubtfully referred to a member of the Theropodous group. The exact age of the deposits in which these reptilian remains are found is doubtful, but is most probably Lower Cretaceous.

The second Memoir, "On Cetacean Skulls from Patagonia," is valuable for the light which it throws on some points in the phylogeny of the group in question. The first form described is a small baleen whale, referred to the European genus *Cetotherium* and remarkable for the large size of the nasals, which partly roof in the narial chamber. Two other European genera, *Physodon* and *Hoplocetus* (*Paracetus*), are represented by skulls, hitherto unknown; and a new family, the Physodontidæ, is instituted for their reception, from the fact that the upper jaw possesses teeth, thus differing from that of the nearly related sperm whales. The Squalodontidæ are represented by a new genus, *Prosqualodon*, in which the nasals are better developed than in *Squalodon*. Moreover, the mandibular symphysis is shorter and the dentition is different, there being five (? six) molars and four premolars. The most interesting form of all is referred to a new genus, *Argyrocetus*, which is made the type of a family, the Argyrocetidæ; it is said to be most nearly related to the Platanistidæ. The notable characteristics of this genus are the possession of large square nasals and prominent occipital condyles, and the complete symmetry of the skull. The nasal opening may have been double. The deposits in which all these remains were found are not later than the Miocene and may be Upper Eocene.

In the third Memoir, "A Study of the Extinct Ungulates of Argentina," the author states that his "main object has been to find out how many of the numerous genera that have been named of late years on the evidence of more or less satisfactory materials were really entitled to stand, and also to elucidate in some degree their natural affinities." He has, however, passed over such genera as are founded on insufficient evidence or are not represented in the

Museum, and does not attempt to give a complete synonymy of those which he recognises as valid.

The suborder Toxodontia is first discussed, and its members are regarded as phylogenetically "related to the Perissodactyla, but as retaining certain features now common to the Artiodactyla, which have probably been inherited from common Condylarthrous ancestors." The suborder is subdivided into three families—the Pachyrucidæ, Typotheridæ, and Toxodontidæ. Of these the first includes two genera, *Pachyrucus* and *Hegetotherium*, the species of which are all of small size, and are characterized by the absence of internal enamel folds in the upper molars, by the antero-posterior elongation of the mandibular condyle, and, in *Pachyrucus* at least, by the possession of perfectly unguiculate feet. The author lays some stress upon the rodent-like appearance of these forms, which, however, he regards merely as the result of convergence. In the Typotheridæ is included the genus *Typotherium* only. The family Toxodontidæ embraces the genera *Toxodon, Toxodontotherium, Xotodon, Stenostephanus,* and *Nesodon.* Under most of these there is a more or less imposing list of synonyms; but the climax is reached in the case of *Nesodon*, of which no less than ten synonymous generic terms are given, while of the species *Nesodon imbricatus* and *N. patagonicus* the specific names are said to be countless. Some explanation of this unfortunate condition of affairs is to be found in the extraordinary changes, here fully described, which are undergone by the dentition at different ages in animals of this genus; but many of the names seem to have been perpetrated without a shadow of excuse.

A new suborder, the *Astrapotheria*, is established for the reception of the families Homalodontotheridæ and Astropotheridæ, which it was found impossible to include in the *Toxodontia, Litopterna,* or *Perissodactyla.*

In the *Litopterna* the author places the Proterotheridæ (including the genera *Diadiaphorus, Epitherium,* and *Proterotherium*) and the Macrauchenidæ (with the genera *Oxyodontotherium, Scalabrinia*—a name substituted for the barbarous compound *Scalabrinitherium*—and *Macrauchenia*). In this latter family the author states that there is a gradual increase in size and specialization from the older Patagonian Tertiaries to the later Pampean deposits.

In the *Perissodactyla* three genera of the Equidæ, *Equus, Hippidium,* and *Onohippidium,* are recognised, Burmeister's opinion that *Hippaplus* is merely founded on worn teeth of *Hippidium* being confirmed. Both *Hippidium* and *Onohippidium* are remarkable for the great backward prolongation of the nasal slits; *Onohippidium* is further notable for possessing enormous lachrymal fossæ.

Ribodon limbatum, a reputed tapir, is now found to be a member of the *Sirenia,* and is perhaps referable to *Halitherium* or *Prorastoma.* The Memoir concludes with an account of some of the *Artiodactyla.*

The plates, some forty-three in number, with which these Memoirs are illustrated are very beautiful examples of what may be done in this direction by photography, but at the same time the

softness of outline and gradation of tone to which their beauty is due, renders it very difficult in many cases to make out details of structure, especially in the teeth.

Mr. Lydekker is paying a second visit to La Plata, and we can only hope that he will make further contributions to our knowledge as valuable as those contained in the present work, and will treat the Edentata and Marsupials as he has here done the Ungulates.

II.—Description de la Faune Jurassique du Portugal. Classe des Céphalopodes. Par Paul Choffat. Première série: Ammonites du Lusitanien de la Contrée de Torres Vedras. Direction des Travaux Géologiques du Portugal. Pp. 1–82, pls. i.–xix. (Lisbonne, 1893.)

THIS is the first part of M. Choffat's Monograph of the Ammonites of the Jurassic rocks of the Torres Vedras region, under which name the author includes the Jurassic rocks of the Montejunto Chain, and those of the less elevated region, bounded on the east by the Tertiary beds of the Tagus basin, and on the south by the Cretaceous rocks extending from the Tagus to the Atlantic. The author states that he has almost completed the stratigraphical description of this region, but as this detailed account has not yet been published, he prefaces his description of the Ammonite fauna with a few remarks on the sequence of the beds.

In the rocks which succeed the Callovian two great divisions can be recognised—the Lower Malm, which the author calls "Lusitanien"; and the Upper Malm, comprising beds corresponding to the Portlandian and Pterocerian of Central Europe, and strata containing a fauna intermediate between that of the latter and that of the Lower Malm. Cephalopods are exceedingly rare in the Upper Malm, but the Lower Malm or Lusitanian has yielded the rich Ammonite fauna described in the present work.

The Lusitanian is divided into three principal divisions, which, in descending order, are (i.) the Abadia series, having a thickness of about 800 metres; (ii.) the Montejunto limestones with a thickness of from 200 to 500 metres; and (iii.) the Cabaço limestones with a thickness of about 500 metres.

The author then proceeds to a description of the species. The Cabaço beds have furnished a few specimens of *Cardioceras*, which cannot, however, be specifically identified. The genus *Phylloceras* is represented by seven species, one being new, viz. *Phylloceras Douvillei*, a species intermediate between *Phyll. subobtusum*, Kudernatsch, and *Phyll. Beneckei*, Zittel. Under *Phyll. silenum*, Fontannes, the author makes some interesting observations on this and allied species, and to this species he refers the form which Sharpe recorded from this region as *Ammonites tortisulcatus*, d'Orbigny. One species of *Lytoceras*, which the author compares with *L. Adeloides* (Kudernatsch), has been furnished by the Montejunto beds. *Harpoceras* is represented by three species, one being new, but not named, whilst *Ochetoceras*, which is regarded as a subgenus of *Harpoceras*, is

represented by two already described species, and a new species (not named) near to *H. canaliferum* (Oppel). Four species of *Oppelia* are recorded; one of these is stated to be new (but not named), and described as near to *O. hectica* (Reinecke). *Neumayria* is represented also by four species, one of which (*N. Kobyi*) is new and closely allied to *Neum. Franciscana*, Fontannes. To this genus M. Choffat assigns the specimen which Sharpe doubtfully referred to *Amm. Boucaultianus*, d'Orbigny. The upper part of the Montejunto beds has yielded some very small specimens of *Olcostephanus*. For the genus *Perisphinctes*, which is numerously represented, the author adopts the classification of Sutner and Steinmann; more than forty species are recognised, seventeen of which are described and named as new. Of the ten species which are recognised as belonging to *Aspidoceras*, one is new (*A. Lusitanicum*). *Peltoceras* is represented by two species, one of which is considered to be new, but is not named. A specifically indeterminable example and a few small specimens are referred to Zittel's genus *Simoceras*, whilst the genus *Hoplites* is represented by the new species *H. Guimarãesi*. Aptychi also have been found.

The Cabaço beds exhibit a decidedly Oxfordian facies; their Ammonite fauna corresponds to that of the *transversarius*-zone, the *cordatus*-zone being represented by about 200 metres of limestones, devoid of Ammonites, separating these Ammonite-bearing beds from the underlying Callovian rocks.

The strata at the base of the Montejunto beds for a thickness of 10 metres yield Lamellibranchs which suggest affinity with those of the Cabaço beds below; but their Ammonite fauna is comparable rather with that of the *bimammatus*-zone. The Ammonite fauna of the rest of the beds presents a mixture of forms belonging to the *bimammatus*- and *tenuilobatus*-zones, thereby resembling the fauna of the *bimammatus*-zone in Central Europe. It possesses a character more Mediterranean than that of Swabia (S. Germany) and of Argovia (Switzerland); but cannot be regarded as the typical Mediterranean facies of this zone; this is found further to the south, in Algarve (Portugal).

The Ammonite fauna of the Abadia series is referable to that of the *tenuilobatus*-zone; the Ammonite-bearing beds do not rest upon Montejunto beds, but are separated from them by strata having a thickness of nearly 400 metres.

M. Choffat deserves our best thanks for so fully describing this rich and interesting fauna, which is amply illustrated in the accompanying twenty excellent phototype plates. G. C. C.

III.—Geological Survey of Canada, Alfred R. C. Selwyn, F.R.S., Director. Annual Report (New Series): Vol. V. 1890–91. (Ottawa: S. E. Dawson, 1893.)

CONSIDERING the wide field embraced in the range of the Geological Survey of Canada, which extends across the continent from the Atlantic to the Pacific, one is not surprised at the bulk of the Annual Report, which now appears in two parts,

containing 1566 pages of text, and various maps and illustrations. From the summary of the Director we learn that geological investigations were carried on in all the provinces of the Dominion by no fewer than twenty distinct parties, of which one was in British Columbia, one in Alberta, two in Manitoba, seven in Ontario, five in Quebec, one in New Brunswick, and three in Nova Scotia. In the Eastern provinces the survey work consists mainly in revising and adding to the details of the geology of districts which have been already examined and reported on, whilst in the North-West and in British Columbia the work is largely in exploring and mapping new areas, and one may naturally look for greater interest and novelty in the reports from these regions.

The first report, after the summary of the Director, is by Mr. R. G. McConnell on the country between the Peace River and the Athabasca River, north of Lesser Slave Lake, comprising an area of about 44,000 square miles, which is for the most part a gently undulating wooded plain, diversified with numerous lakes, muskegs, and marshes. The lakes are mostly shallow hollows in Boulder-clay; many of those of former times are now filled up. The greater part of the region is underlain by soft dark greyish or brownish shales of Cretaceous age, with but few fossils in them. They have an estimated thickness of 3000 feet, and they represent the Dakota, Colorado, Montana, and Laramie formations. The Cretaceous beds rest directly on nodular and crumbly, nearly horizontal, limestones, filled with fossils of Devonian age. Not more than 100 feet of these limestones are exposed in the Peace and Athabasca regions. The most striking geological feature of this district is the so-called Tar-sands, exposed in the valley of the Athabasca River and some of its tributaries. The Tar-sands were originally unconsolidated sands and soft sandstones, ranging from fine silt to coarse grit, and they are now thoroughly impregnated by a bituminous material which has cemented them into a coherent tarry mass. In places they form high cliffs, varying from dark-brown to jet black, from which streams of so-called tar issue in warm weather and form pools at the base of the escarpment. The Tar-sands are exposed for a distance of 90 miles along the Athabasca Valley; they are from 140 to 225 feet in thickness, and have an estimated minimum distribution of 1000 square miles. The sands themselves are clearly of Cretaceous age, of which they form the lowest local division, and they rest directly on the greyish, crumbly, evenly stratified, Devonian limestones. The enormous amount of bituminous material with which the sands are now charged is considered by the Canadian geologists to be the heavy constituents of petroleum oil which has welled up from the underlying Devonian limestones during past ages. But, strangely enough, these limestones, so far as they are exposed, are by no means specially bituminous in character, and the sources of the bituminous material in the sands are therefore attributed to underlying beds, of which at present nothing is known.

The Athabasca and Peace River regions are so thickly covered with Glacial Drift that it is only in the main drainage valleys and

along the escarpments of some of the plateaux that the older rocks are shown. The basal glacial beds are stratified sands and gravels overlain by Boulder-clay, and this in places is covered by drift gravels. In some of these latter there are masses of the Tar-sands, which have been transported by the glacier, and Mr. McConnell discovered portions of this material on the plateau summit of Birch Mountain, which could only have been derived from the exposures in the Athabasca Valley at a lower level of many hundreds of feet. Accompanying the report of this district are several photographs, an index map, and sections.

Another elaborate and interesting report is that by Mr. J. B. Tyrrell on North-Western Manitoba, including portions of the adjacent districts of Assineboia and Saskatchewan. The oldest rocks of this district are Silurian, mainly limestones and dolomites, with characteristic fossils of Niagara age, which are shown near the mouth of the Saskatchewan on the west side of Lake Winnipeg, and also along the eastern shores of Lakes Winnepegosis and Manitoba. On the western borders of these lakes there is an extended belt of Devonian rocks, from 400 to 500 feet in thickness: the lower portion of these are red shales, which are overlain by thick beds of dolomites containing numerous fossils, amongst others the well-known brachiopod *Stringocephalus Burtini*, and these are succeeded by limestones and shales of Upper Devonian age. Brine springs are met with in certain areas of the Devonian, west of L. Winnepegosis.

As in the Saskatchewan region, 500 miles further west, there is here also a tremendous break in the geological succession, so that the Devonian rocks are immediately succeeded by Cretaceous shales and sandstones, of which the country to the west of the lake region is mainly formed. The lowest beds are Dakota sandstones, about 200 feet thick; these are overlain by very dark shales of Benton age, about 180 feet in thickness, followed by grey calcareous marls or shales, occasionally passing into limestones, belonging to the Niobrara division. These beds contain numerous Foraminifera, amongst which *Globigerina cretacea* is the most conspicuous species. Some layers are largely composed of the microscopic prisms of disintegrated shells of *Inoceramus*. The scales, teeth, and bony fragments of fishes are likewise abundant in these Niobrara shales, and Mr. Tyrrell discovered a thin band or bone-bed largely made up of these remains, which yielded on analysis 37 per cent. of phosphate of lime. Above the Niobrara beds are the grey soft shales of the Pierre division, which occupy the higher lands of the Riding, Duck, and Porcupine mountains, and reach a thickness of about 1000 feet. Some of these shales are highly siliceous, and they contain numerous Radiolaria, which have been determined by Dr. Rüst.

The harder rocks round the lakes of this district show very distinctly the glacial striæ, the direction of which ranges for the most part to the west of south. As an instance of the way in which the course of the glacier has overridden the slope of the country, Mr. Tyrrell records the occurrence of large boulders of Dakota sandstone which have been carried to a level of 450 feet above that of the parent rock, the nearest bed of which is 40 miles distant.

The next report is that of Dr. Robert Bell on the Sudbury Mining District to the north of Lake Huron, now noted for the presence of nickel and copper ores, which are extensively worked. The district is wild, hilly, and rocky, and the glacial boulders are often piled one on the top of another without any fine material between them. The rock succession is: (1) Laurentian gneiss and hornblende granite; (2) Huronian quartzites, greywackes, and dolomites; (3) Dark-coloured siliceous volcanic breccias and black slates, overlain by argillaceous beds and nearly black gritty sandstones probably of Upper Huronian or Cambrian age. The nickel and copper ores are always found in intimate association with the greenstones, and generally at their junction with some other rock. The microscopical characters of the Huronian rocks of this district are fully described in an appendix by Prof. G. H. Williams.

Passing over Mr. A. J. Low's Report on the Geology of three counties near Quebec, and of Mr. Hugh Fletcher on that of the counties of Picton and Colchester, Nova Scotia, we find a lengthy report on Natural Gas and Petroleum in Ontario prior to 1891 drawn up by Mr. P. H. Brumell. Since the remarkable discoveries of natural gas in the Trenton rocks of Ohio, numerous borings in search of this product have been carried out in various parts of Ontario, but the only area in which it has been found in any quantity is that between Lakes Ontario and Erie, not far from the Welland Canal. The gas occurs in a sandstone of the Medina formation (Silurian), which is here from 800 to 900 feet beneath the surface. The only petroleum-producing district in Ontario is in the county of Lambton, and the oil is found in the Corniferous limestone (Devonian) at depths from 370 to 480 feet beneath the surface. The various borings have yielded an interesting record of the distribution and depth of the Palæozoic rocks in Southern Ontario, and they show a possible maximum thickness of 4200 feet overlying the Laurentian gneiss.

A considerable portion of part ii. of the volume is occupied by a chemical contribution to the Geology of Canada by G. C. Hoffmann, and by the reports of mineral statistics and mines, which are mainly of economical importance.

The value attached by the Government of the Dominion to the Geological Survey of the country is satisfactorily shown in this Annual Report; and we are glad to notice that it is published at the moderate price of two dollars, or less than eight shillings, whilst the particular reports, and the maps, can be obtained separately at prices from sixpence to one shilling each. G. J. H.

IV.—Chinese Central Asia: a Ride to Little Thibet. By Henry Lansdell, D.D., etc. (With Three Maps and Eighty Illustrations.) In Two Volumes. London: Sampson Low, Marston, and Co. 1893.

DR. LANSDELL'S adventurous journey across the western end of Chinese Central Asia was undertaken in connection with mission work, but his book incidentally includes a considerable

amount of information, more or less geological. Starting from Kuldja, after a visit to Lake Issik-kul, he crossed the Thian Shan Chain by a glacier pass—the Muz-davan—which is probably from 11,000 to 12,000 feet above the sea, and never before had been completely traversed by a European. Thence he made his way to Kashgar and Khotan in Chinese Turkestan, and finally crossed to Leh by the Kilian, Suget, Karakoram, and Saser Passes. All these are more than 17,000 feet above the sea-level, the Karakoram being 18,550 feet. The chief points of geological interest in the book are accounts of a severe earthquake at Vierny, near the Ala-tau Mountains, and of the geology of Russian Turkistan, compiled from Romanovsky and Mushketoff's map, and the descriptions of the high passes over which his journey lay. Unfortunately the exigencies of travel prevented Dr. Lansdell from forming a collection of rock-specimens, but from his own observations, and by availing himself of Russian writings, he is able to give some notion of the geology, and still more of the geography, of Turkestan and the bordering mountain chains. The book, in short, will be a valuable work of reference on districts which, till lately, were practically inaccessible to Europeans.

REPORTS AND PROCEEDINGS.

I.—May 23rd, 1894.—Dr. Henry Woodward, F.R.S., President, in the Chair. The following communications were read:—

1. "On the Stratigraphy and Physiography of the Libyan Desert of Egypt." By Captain H. G. Lyons, R.E., F.G.S.

The Nubian sandstone, wherever seen, rests unconformably on the old rocks called by Sir J. W. Dawson Archæan, and the author finds no case of alteration of sandstone by these rocks, though in one case it is altered by an intrusive dolerite.

The author considers the Nubian sandstone to be an estuarine deposit which was formed on an area afterwards gradually invaded by the Cretaceous sea. He considers the whole of the sandstone in the region which he has examined to be of Cretaceous age.

He describes a series of anticlinals, one set running W.N.W.–E.S.E., and the other N. by E. and S. by W. Many springs of the oases seem to occur along these anticlinals, owing to the beds which contain the water being brought nearer to the surface. Historical evidence is discussed which points to the Nile having reached a higher level in Nubia than it does at present, and it is suggested that variations in the level of the river were caused by earth-movement opposing obstructions to the river's flow.

The sandstone of Jebel Ahmar near Cairo is described, and its occurrence over a wide area of west Cairo is recorded. The author considers its age to be later Miocene. He believes that, with the exception of some erosion after the deposition of the Eocene beds, the greatest erosion, including the cutting out of the Nile Valley, took place in Miocene times, while a certain amount, bringing the area to its present condition, was done in Quaternary times.

This agrees with the observation of the French geologist in Algeria. The origin of the silicification of the fossil trees of the sandstone deposit is discussed, and the action of water containing sodium carbonate suggested as a cause.

2. "Notes on the Geology of South Africa." By D. Draper, Esq., F.G.S.

The district here considered includes Natal, Zululand, Swaziland, the S.E. part of the Transvaal, and the Eastern part of the Orange Free State and Basutoland. Physically it comprehends:—1. The Drakensberg Range; divided into—*a*, Mountain portion; *b*, Hill-covered plateau; *c*, Highveld plateau: 2. The terrace along its foot: 3. The coast-belt. Their main features and characteristics are described. The geological formations are:—

Karoo Beds.	Upper.	1. Volcanic Beds.
		2. Cave Sandstone.
		3. Red Beds.
		4. Molteno Beds.
	Lower.	5. Beaufort Beds.
		6. Ecca Beds.
		7. Dwyka (Ecca) Conglomerate.
		[Bokkeveld Beds, wanting.]
	Palæozoic.	8. Gats Rand (Zuurberg) Quartzite.
		9. Dolomitic Limestone.
		10. Table-Mountain Sandstone.
		11. Malmesbury Schists.
		12. Gneiss and Granite.

No. 1 briefly noticed. 2. This caps the hills of the plateau (1, *b*); and has yielded fossil fishes (described by Mr. A. S. Woodward, F.G.S.) in the Orange Free State. 3. These are exposed in the O. F. S., at Harrismith, about 100 feet thick, and containing a bone-breccia from which reptilian remains were described by Prof. Owen in 1854. Northwards the Red beds change to a dark grit; and siliceous tree-stumps occur in the upper part. 4. These are the well-known coal-bearing beds of the Highveld plateau (1, *c*) and Natal, including the terrace (2) north of the Tugela River, and Zululand. The lower 500 feet of these beds in Natal carry coal better than that of the Highveld, but have been let down to a lower level (about 2000 feet). In Natal they thicken northwards. On the coast-line in Natal and Zululand portions are at a still lower level and dip seaward at 20°. Anthracite occurs at St. Lucia Bay. 5. These form hill-side crags along the edge of the terrace (2); and die away northwards near the Pongolo River. 6. The "Pietermaritzburg Shales" of Dr. Sutherland; also die out northwards near that river. 7. The "Glacial Conglomerate" of Dr. Sutherland; exposed in high crags in the deeper gorges of the terrace (2); stratified and ripple-marked; horizontal inland, but near the coast dipping seaward with the overlying beds. A patch, much rippled and containing few pebbles or boulders, lies horizontal in the coast-belt (3) of Zululand and Swaziland at least 1000 feet lower than the main body seen along the terrace (2). No. 7 stretches from St. John's River, through Pondoland and Natal to Zululand, thins out and disappears near the Pongolo. Intrusive and flat diorites

were noticed in it. A definite list of the strata of this conglomerate, where it is 138 feet thick in Zululand, is given; and the author does not regard it as of glacial origin. No 9 is treated of in the next paper.

3. "On the Occurrence of Dolomite in South Africa." By D. Draper, Esq., F.G.S.

A peculiar calcareo-siliceous rock, near Lydenburg, described by Messrs. Penning and Crutwell as "Chalcedolite," and a similar rock mentioned by Mr. Penning, F.G.S., as overlying the "Black reef Series" of the Megaliesberg formation, are recognised as dolomite. Mr. C. Alford, F.G.S., has described a "calcareous quartzite," as passing into dolomite and ultimately into chert, and known as the "Elephant rock" in Transvaal, sometimes cavernous with underground waters. From his own experience Mr. Draper has recognised the "Elephant rock" in the Potschefstroom, Lichtenburg, Malmani, and Lydenburg districts as a real dolomite, with interstratified siliceous bands, weathering into a brown earth like manganese oxide, and superficial siliceous débris. It has its place between the Table-mountain Sandstone and the quartzite of the Gats Rand (=Zuurberg quartzite of the Cape). It has auriferous veins in Malmani and Lydenburg. Dr. Schrenck has noticed a similar dark blue dolomitic limestone in Great Namaqualand. The deep water holes in it in Malmani are comparable with those found by F. Galton in West Central Africa. The great caves in Mashonaland may belong to it. The extensive tufaceous deposits in Griqualand-West, the Transvaal, and Orange Free State were probably derived from this extensive dolomite.

4. "Contributions to the Geology of British East Africa." By J. W. Gregory, D.Sc., F.G.S.

The author describes moraines, striæ, glacial lake-basins, perched blocks, and *roches moutonnées* below the present limits of the glaciers of Mount Kenya, which he maintains to indicate the existence of a "calotte" or ice-cap extending at least 5,400 feet farther down the mountain than the termination of the present glaciers, and possibly farther, for in the belt of forest detailed observations could not be made.

He agrees that this more extensive glaciation was produced by a greater elevation of Mount Kenya, and that any theory of universal glaciation is unnecessary, and indeed opposed by many facts in African geology.

He discusses the probable influence of this former glaciation on the meteorological conditions of the surrounding area and the distribution of its flora and fauna.

II.—June 6th, 1894.—Dr. Henry Woodward, F.R.S., President, in the Chair. The following communications were read:—

1. "On the Banded Structure of some Tertiary Gabbros in the Isle of Skye." By Sir Archibald Geikie, LL.D., D.Sc., F.R.S., F.G.S., and J. J. H. Teall, Esq., M.A., F.R.S., Sec.G.S.

After calling attention to the previous references to the pseudo-

bedding and banding of the gabbro-masses of the Inner Hebrides, the authors describe the rocks which form the rugged ridge of Druim-an-Eidhne, near the head of Glen Sligachan. This ridge is made up of parallel beds, sheets, or sills disposed in a general N.N.W. direction with a prevalent easterly dip. Four distinct types of gabbro occur:—(1) dark, fine-grained, granulitic gabbros; (2) well-banded gabbros; (3) coarse-grained massive gabbros; and (4) pale veins of a highly felspathic gabbro. The relative ages of the banded and granulitic gabbros have not been definitely settled; but the coarse massive gabbros are certainly intrusive in the banded series, and the pale veins cut all the other varieties.

The paper deals mainly with the banded gabbros. They occur in successive sheets or sills which vary from a few feet to many yards in thickness, and consist of parallel layers of lighter and darker material which correspond in direction with the trend of the sheets, and are usually inclined to the east or south-east at angles ranging from 20° to 30°. In some cases the bands can be seen to have been puckered or folded.

The minerals entering into the composition of the banded, as also of the other varieties, are labradorite, pyroxene, olivine, and titaniferous magnetite. The banding is due to a variation in the relative proportions of the different constituents, and especially in the amount of magnetite. Some narrow bands and lenticles are composed entirely of pyroxene and magnetite. The variations in chemical composition are illustrated by three analyses by Mr. Player. The microscopic characters of the rocks are described, and it is shown that the minerals of the banded gabbros have not been crushed or broken since they were formed.

The authors conclude that the banding is the result of the intrusion of a heterogeneous magma, and that similar banding in certain portions of the Lewisian gneiss may have been produced in the same way.

2. "On the Microscopical Structure of the Derbyshire Carboniferous Dolerites and Tuffs." By H. H. Arnold-Bemrose, Esq., M.A., F.G.S.

The paper deals with the petrography of the Toadstones or igneous rocks of Derbyshire. Brief reference is made to the work of previous petrographers, the age of the rocks, and the question as to the number of beds. The outcrops mapped by the Geological Survey, and several additional ones, have been examined, and the results given in a table for the purpose of the paper and for future reference.

The Toadstone is divided into massive rocks or lavas, and fragmental rocks or tuffs. The former consist of olivine-dolerite, either with granular or with ophitic augite, and olivine-basalt. The rock is often very fresh, but in some places is altered to a diabase. The principal constituent minerals are described. A pseudomorph of olivine, optically like biotite and somewhat like Iddingsite but differing from it chemically, is fully described.

The latter portion of the paper deals with the tuffs, which are

much more extensive than has been hitherto supposed. Specimens are described, taken from thirteen outcrops.

3. "On the Origin of the Permian Breccias of the Midlands, and a Comparison of them with the Upper Carboniferous Glacial Deposits of India and Australia." By R. D. Oldham, Esq., F.G.S.

The author first describes the Permian breccias of the Midland Counties of England, which he had the opportunity of examining at Eastertide of the present year. He describes the characters of the breccias, and concludes that they were formed subaërially as gravel-fans by rivers charged with a maximum load of sediment, and therefore incapable of performing any appreciable amount of erosion. An examination of many of the fragments at Abberley and some at Church Hill reveals the presence of scratches, which occur in such a manner that the author believes they existed on the fragments before they were transported, and discusses the evidence for their production by ice or soil-cap movement, deciding in favour of the former.

A short description of the Upper Carboniferous deposits of India follows, and it is pointed out that they differ markedly from the deposits of Britain. Amongst other things the separation of different pebbles by considerable interspace of matrix, and the bending of stratification-planes round a pebble as though the pebble had dropped from above, are noted, and it is maintained that floating ice alone will account for these pebbles being dropped into the Indian deposits. Finally, it is remarked that the so-called Upper Carboniferous deposits of India and the Permian deposits of the Midlands of Britain may be practically contemporaneous, as maintained by the late Mr. H. F. Blanford, indicating a possible simultaneous existence of glaciers in England, India, and Australia.

CORRESPONDENCE.

NORWEGIAN ROCKS IN THE ENGLISH BOULDER-CLAYS.

Sir,—Anyone familiar with the Boulder-clays of our East Coast—or, I may add, with the methods of working customary among field-geologists—must have read with astonishment Sir Henry Howorth's confident suggestion that the records of "so-called Norwegian boulders" are due to material brought by ships as ballast. In the first place, he clearly has no idea of the immense profusion of these boulders, hundreds of which may be observed in as many yards on some parts of the Holderness beach. This is in places where the Basement Clay is exposed in the cliffs and to the south of such places, *i.e.* in the direction of movement of the beach. If, however, Sir H. Howorth can find an adequate explanation of this in the statistics of ship-wrecks, he still has to meet the fact that these boulders are found not only on the beach but in the clay. Five years ago I examined and described specimens of the Laurvig augite-syenite collected by Mr. Lamplugh from the Basement Clay of Dimlington and Bridlington Quay. This was merely that my

friend and myself might be perfectly satisfied of the identification of this rock-type. The fact that such rocks occur as boulders in the clay had long been a matter of common knowledge, and half an hour's work at the cliffs will always produce specimens of this augite-syenite and of the equally characteristic rhomb-porphyry, the two most striking rocks among the beach material. Surely it would be "more in accordance with scientific laws of evidence" to ascertain the facts of the case, either personally or from the records, before propounding an artificial explanation of them.

If further evidence be needed, I may add that I have recently sliced and examined typical specimens of the two unique Scandinavian rocks mentioned above, which were collected at Cambridge by Professor Hughes.

St. John's College, Cambridge,
June 2nd, 1894. Alfred Harker.

UNIFORMITY IN GEOLOGY AND THE ORIGIN OF THE DRIFT.

Sir,—Sir Henry H. Howorth opens a rather strangely reasoned paper in your last month's issue with the statement that signs are accumulating everywhere that geologists are now harking back to the views of the old catastrophists, and giving up the uniformitarian views so ably placed on record by Lyell and later workers. Where these signs are to be seen I am at a loss to discover. Certainly not in the Nottingham Address of Mr. J. J. Harris Teall to the geological section of the British Association, nor yet in the text-books and original papers written during the last few years. No doubt popular magazine writers will to some extent regard Sir Henry H. Howorth's writings as being one of the signs of the times, and will be ready to put his ideas before their readers as "recent advances." Every supporter of uniformitarian principles admits that floods and earthquakes have always occurred. Nor am I aware that any exact limit has been fixed to their magnitude. At least I never heard it argued that the eruption of Krakatoa, for example, was the greatest outburst that has ever occurred, or that there will never be a greater. It only asks us to seek to explain the facts by slow and well-known causes that may be seen in every-day action rather than by extreme or violent means. For instance, the great majority of geologists consider that the distribution of the drifts can be best accounted for on the assumption that large portions of the northern continents were covered by immense ice-fields. Judging from the present distribution of glaciers and ice-fields, etc., Sir Henry H. Howorth thinks this view extreme and not sufficiently uniformitarian, and pins his faith on floods and dancing mountain ranges. The time has gone by for a general discussion on this point. If a particular deposit can only be explained on the assumption that there was a deluge, we must believe that there was a deluge. It would be unscientific to settle upon the agent first and then point to all sorts of deposits as being produced by it.

But, to return to the paper, my intention was also to refer to

two letters which appeared in "Nature," and which Sir Henry H. Howorth quotes. One was from the pen of Professor Bonney. In it he asks whether the fact that there is a deep submarine channel along the coast of Norway does not render it improbable that the ice-sheet could have crossed it and reached Great Britain. Professor Bonney very wisely does not say that it could not, for he would be a rash man who would say what the snowfall was upon the mountains, and how far the ice would have to move from them before it melted, and the law of supply and wasting was satisfied. However, on the strength of the fact that the question has been asked, we learn that "Prof. Bonney has added a new and striking" objection, "based upon the difficulty an ice-sheet would have in traversing the hollow trough."

Another letter, also published in "Nature," and written by Prof. Hughes, is also referred to. In this letter Prof. Hughes calls attention to the fact that on such coasts as that of Norfolk great care should be taken, when collecting rock-specimens from the drift, that the boulders are really from the drift, and not ballast thrown overboard or spread along the coast by the wreck of a vessel. When I read the letter I thought it was addressed as a warning to some careless student, but we now hear from Sir Henry H. Howorth that it is an expression of opinion on the part of Prof. Hughes that the officers of the Geological Survey, and other experienced field geologists, who have visited the coast have not been alive to such possibilities.

I should like to notice the rest of the paper, but to do so would require too much space, Sir Henry H. Howorth having treated the drifts as a hotch-potch of mud, sand, and gravel, instead of a series of deposits formed at different times and under different circumstances.

R. M. Deeley.

JURASSIC SPECIES OF CHEILOSTOMATA.

Sir,—My attention has been drawn to Mr. Edwin A. Walford's articles "On some Bryozoa from the Inferior Oolite of Shipton Gorge," and "On Cheilostomatous Bryozoa from the Middle Lias," both of which were published in the Quarterly Journal of the Geological Society, Vol. L., February, 1894, pp. 72–82.

I regret that I have omitted to make a reference to Mr. Walford's papers in an article published by me, "On some Jurassic species of Cheilostomata" in the Geological Magazine for February, 1894, pp. 61–64; but at that time Mr. Walford's papers were unpublished save for the abstracts which had appeared in April and June, 1893, during my absence in East Africa.

J. W. Gregory.

Errata.—Sir H. H. Howorth begs us to correct the following errata in his article in the June Number of this Magazine:—

On p. 260, line 41 from top, for "Hampshire" read *Sussex*.
On p. 262, line 8 from top, for "Micael" read *Michel*.
On p. 262, line 31 from top, delete the words "among them."